BRITISH ASSOCIATION
FOR THE ADVANCEMENT OF SCIENCE

REPORT

OF THE

NINETIETH MEETING

HULL—1922

SEPTEMBER 6–13

LONDON
JOHN MURRAY, ALBEMARLE STREET

OFFICE OF THE ASSOCIATION
BURLINGTON HOUSE, LONDON, W.1

1923

CONTENTS.

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CONFERENCE OF DELEGATES OF CORRESPONDING SOCIETIES.

President.—W. Whitaker, F.R.S.

TABLE OF

Date of Meeting	Where held	Presidents	Old Life Members	New Life Members
1831, Sept. 27	York	Viscount Milton, D.C.L., F.R.S.	—	—
1832, June 19	Oxford	The Rev. W. Buckland, F.R.S.	—	—
1833, June 25	Cambridge	The Rev. A. Sedgwick, F.R.S.	—	—
1834, Sept. 8	Edinburgh	Sir T. M. Brisbane, D.C.L., F.R.S.	—	—
1835, Aug. 10	Dublin	The Rev. Provost Lloyd, LL.D., F.R.S.	—	—
1836, Aug. 22	Bristol	The Marquis of Lansdowne, F.R.S.	—	—
1837, Sept. 11	Liverpool	The Earl of Burlington, F.R.S.	—	—
1838, Aug. 10	Newcastle-on-Tyne	The Duke of Northumberland, F.R.S.	—	—
1839, Aug. 26	Birmingham	The Rev. W. Vernon Harcourt, F.R.S.	—	—
1840, Sept. 17	Glasgow	The Marquis of Breadalbane, F.R.S.	—	—
1841, July 20	Plymouth	The Rev. W. Whewell, F.R.S.	169	65
1842, June 23	Manchester	The Lord Francis Egerton, F.G.S.	303	169
1843, Aug. 17	Cork	The Earl of Rosse, F.R.S.	109	28
1844, Sept. 26	York	The Rev. G. Peacock, D.D., F.R.S.	226	150
1845, June 19	Cambridge	Sir John F. W. Herschel, Bart., F.R.S.	313	36
1846, Sept. 10	Southampton	Sir Roderick I. Murchison, Bart., F.R.S.	241	10
1847, June 23	Oxford	Sir Robert H. Inglis, Bart., F.R.S.	314	18
1848, Aug. 9	Swansea	The Marquis of Northampton, Pres. R.S.	149	3
1849, Sept. 12	Birmingham	The Rev. T. R. Robinson, D.D., F.R.S.	227	12
1850, July 21	Edinburgh	Sir David Brewster, K.H., F.R.S.	235	9
1851, July 2	Ipswich	G. B. Airy, Astronomer Royal, F.R.S.	172	8
1852, Sept. 1	Belfast	Lieut.-General Sabine, F.R.S.	164	10
1853, Sept. 3	Hull	William Hopkins, F.R.S.	141	13
1854, Sept. 20	Liverpool	The Earl of Harrowby, F.R.S.	238	23
1855, Sept. 12	Glasgow	The Duke of Argyll, F.R.S.	194	33
1856, Aug. 6	Cheltenham	Prof. C. G. B. Daubeny, M.D., F.R.S.	182	14
1857, Aug. 26	Dublin	The Rev. H. Lloyd, D.D., F.R.S.	236	15
1858, Sept. 22	Leeds	Richard Owen, M.D., D.C.L., F.R.S.	222	42
1859, Sept. 14	Aberdeen	H.R.H. The Prince Consort	184	27
1860, June 27	Oxford	The Lord Wrottesley, M.A., F.R.S.	286	21
1861, Sept. 4	Manchester	William Fairbairn, LL.D., F.R.S.	321	113
1862, Oct. 1	Cambridge	The Rev. Professor Willis, M.A., F.R.S.	239	15
1863, Aug. 26	Newcastle-on-Tyne	Sir William G. Armstrong, C.B., F.R.S.	203	36
1864, Sept. 13	Bath	Sir Charles Lyell, Bart., M.A., F.R.S.	287	40
1865, Sept. 6	Birmingham	Prof. J. Phillips, M.A., LL.D., F.R.S.	292	44
1866, Aug. 22	Nottingham	William R. Grove, Q.C., F.R.S.	207	31
1867, Sept. 4	Dundee	The Duke of Buccleuch, K.C.B., F.R.S.	167	25
1868, Aug. 19	Norwich	Dr. Joseph D. Hooker, F.R.S.	196	18
1869, Aug. 18	Exeter	Prof. G. G. Stokes, D.C.L., F.R.S.	204	21
1870, Sept. 14	Liverpool	Prof. T. H. Huxley, LL.D., F.R.S.	314	39
1871, Aug. 2	Edinburgh	Prof. Sir W. Thomson, LL.D., F.R.S.	246	28
1872, Aug. 14	Brighton	Dr. W. B. Carpenter, F.R.S.	245	36
1873, Sept. 17	Bradford	Prof. A. W. Williamson, F.R.S.	212	27
1874, Aug. 19	Belfast	Prof. J. Tyndall, LL.D., F.R.S.	162	13
1875, Aug. 25	Bristol	Sir John Hawkshaw, F.R.S.	239	36
1876, Sept. 6	Glasgow	Prof. T. Andrews, M.D., F.R.S.	221	35
1877, Aug. 15	Plymouth	Prof. A. Thomson, M.D., F.R.S.	173	19
1878, Aug. 14	Dublin	W. Spottiswoode, M.A., F.R.S.	201	18
1879, Aug. 20	Sheffield	Prof. G. J. Allman, M.D., F.R.S.	184	16
1880, Aug. 25	Swansea	A. C. Ramsay, LL.D., F.R.S.	144	11
1881, Aug. 31	York	Sir John Lubbock, Bart., F.R.S.	272	28
1882, Aug. 23	Southampton	Dr. C. W. Siemens, F.R.S.	178	17
1883, Sept. 19	Southport	Prof. A. Cayley, D.C.L., F.R.S.	203	60
1884, Aug. 27	Montreal	Prof. Lord Rayleigh, F.R.S.	235	20
1885, Sept. 9	Aberdeen	Sir Lyon Playfair, K.C.B., F.R.S.	225	18
1886, Sept. 1	Birmingham	Sir J. W. Dawson, C.M.G., F.R.S.	314	25
1887, Aug. 31	Manchester	Sir H. E. Roscoe, D.C.L., F.R.S.	428	86
1888, Sept. 5	Bath	Sir F. J. Bramwell, F.R.S.	266	36
1889, Sept. 11	Newcastle-on-Tyne	Prof. W. H. Flower, C.B., F.R.S.	277	20
1890, Sept. 3	Leeds	Sir F. A. Abel, C.B., F.R.S.	259	21
1891, Aug. 19	Cardiff	Dr. W. Huggins, F.R.S.	189	24
1892, Aug. 3	Edinburgh	Sir A. Geikie, LL.D., F.R.S.	280	14
1893, Sept. 13	Nottingham	Prof. J. S. Burdon Sanderson, F.R.S.	201	17
1894, Aug. 8	Oxford	The Marquis of Salisbury, K.G., F.R.S.	327	21
1895, Sept. 11	Ipswich	Sir Douglas Galton, K.C.B., F.R.S.	214	13
1896, Sept. 16	Liverpool	Sir Joseph Lister, Bart., Pres. R.S.	330	31
1897, Aug. 18	Toronto	Sir John Evans, K.C.B., F.R.S.	120	8
1898, Sept. 7	Bristol	Sir W. Crookes, F.R.S.	281	19
1899, Sept. 13	Dover	Sir Michael Foster, K.C.B., Sec.R.S.	296	20

* Ladies were not admitted by purchased tickets until 1843. † Tickets of Admission to Sections only.

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ANNUAL MEETINGS.

Old Annual Members	New Annual Members	Associates	Ladies	Foreigners	Total	Amount received for Tickets	Sums paid on account of Grants for Scientific Purposes	Year
—	—	—	—	—	353	—	—	1831
—	—	—	—	—	—	—	—	1832
—	—	—	—	—	900	—	—	1833
—	—	—	—	—	1298	—	£20 0 0	1834
—	—	—	—	—	—	—	167 0 0	1835
—	—	—	—	—	1350	—	435 0 0	1836
—	—	—	—	—	1840	—	922 12 6	1837
—	—	—	1100*	—	2400	—	932 2 2	1838
—	—	—	—	34	1438	—	1595 11 0	1839
—	—	—	—	40	1353	—	1546 16 4	1840
46	317	—	60*	—	891	—	1235 10 11	1841
75	376	33†	331*	28	1315	—	1449 17 8	1842
71	185	—	160	—	—	—	1565 10 2	1843
45	190	9†	260	—	—	—	981 12 8	1844
94	22	407	172	35	1079	—	831 9 9	1845
65	39	270	196	36	857	—	685 16 0	1846
197	40	495	203	53	1320	—	208 5 4	1847
54	25	376	197	15	819	£707 0 0	275 1 8	1848
93	33	447	237	22	1071	963 0 0	159 19 6	1849
128	42	510	273	44	1241	1085 0 0	345 18 0	1850
61	47	244	141	37	710	620 0 0	391 9 7	1851
63	60	510	292	9	1108	1085 0 0	304 6 7	1852
56	57	367	236	6	876	903 0 0	205 0 0	1853
121	121	765	524	10	1802	1882 0 0	380 19 7	1854
142	101	1094	543	26	2133	2311 0 0	480 16 4	1855
104	48	412	346	9	1115	1098 0 0	734 13 9	1856
156	120	900	569	26	2022	2015 0 0	507 15 4	1857
111	91	710	509	13	1698	1931 0 0	618 18 2	1858
125	179	1206	821	22	2564	2782 0 0	684 11 1	1859
177	59	636	463	47	1689	1604 0 0	766 19 6	1860
184	125	1589	791	15	3138	3944 0 0	1111 5 10	1861
150	57	433	242	25	1161	1089 0 0	1293 16 6	1862
154	209	1704	1004	25	3335	3640 0 0	1608 3 10	1863
182	103	1119	1058	13	2802	2965 0 0	1289 15 8	1864
215	149	766	508	23	1997	2227 0 0	1591 7 10	1865
218	105	960	771	11	2303	2469 0 0	1750 13 4	1866
193	118	1163	771	7	2444	2613 0 0	1739 4 0	1867
226	117	720	682	45‡	2004	2042 0 0	1940 0 0	1868
229	107	678	600	17	1856	1931 0 0	1622 0 0	1869
303	195	1103	910	14	2878	3096 0 0	1572 0 0	1870
311	127	976	754	21	2463	2575 0 0	1472 2 6	1871
280	80	937	912	43	2533	2649 0 0	1285 0 0	1872
237	99	796	601	11	1983	2120 0 0	1685 0 0	1873
232	85	817	630	12	1951	1979 0 0	1151 16 0	1874
307	93	884	672	17	2248	2397 0 0	960 0 0	1875
331	185	1265	712	25	2774	3023 0 0	1092 4 2	1876
238	59	446	283	11	1229	1268 0 0	1128 9 7	1877
290	93	1285	674	17	2578	2615 0 0	725 16 6	1878
239	74	529	349	13	1404	1425 0 0	1080 11 11	1879
171	41	389	147	12	915	899 0 0	731 7 7	1880
313	176	1230	514	24	2557	2689 0 0	476 8 1	1881
253	79	516	189	21	1253	1286 0 0	1126 1 11	1882
330	323	952	841	5	2714	3369 0 0	1083 3 3	1883
317	219	826	74	26 & 60 H.§	1777	1855 0 0	1173 4 0	1884
332	122	1053	447	6	2203	2256 0 0	1385 0 0	1885
428	179	1067	429	11	2453	2532 0 0	995 0 6	1886
510	244	1985	493	92	3838	4336 0 0	1186 18 0	1887
399	100	639	509	12	1984	2107 0 0	1511 0 5	1888
412	113	1024	579	21	2437	2441 0 0	1417 0 11	1889
368	92	680	334	12	1775	1776 0 0	789 16 8	1890
341	152	672	107	35	1497	1664 0 0	1029 10 0	1891
413	141	733	439	50	2070	2007 0 0	864 10 0	1892
328	57	773	268	17	1661	1653 0 0	907 15 6	1893
435	69	941	451	77	2321	2175 0 0	583 15 6	1894
290	31	493	261	22	1324	1236 0 0	977 15 5	1895
383	139	1384	873	41	3181	3228 0 0	1104 6 1	1896
286	125	682	100	41	1362	1398 0 0	1059 10 8	1897
327	96	1051	639	33	2446	2399 0 0	1212 0 0	1898
324	68	548	120	27	1403	1328 0 0	1430 14 2	1899

‡ Including Ladies. § Fellows of the American Association were admitted as Hon. Members for this Meeting.

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Table of

Date of Meeting	Where held	Presidents	Old Life Members	New Life Members
1900, Sept. 5	Bradford	Sir William Turner, D.C.L., F.R.S. ...	267	13
1901, Sept. 11	Glasgow..................	Prof. A. W. Rücker, D.Sc., Sec.R.S. ..	310	37
1902, Sept. 10	Belfast	Prof. J. Dewar, LL.D., F.R.S.	243	21
1903, Sept. 9	Southport	Sir Norman Lockyer, K.C.B., F.R.S.	250	21
1904, Aug. 17.....	Cambridge...............	Rt. Hon. A. J. Balfour, M.P., F.R.S.	419	32
1905, Aug. 15......	South Africa	Prof. G. H. Darwin, LL.D., F.R.S. ...	115	40
1906, Aug. 1	York	Prof. E. Ray Lankester, LL.D., F.R.S	322	10
1907, July 31	Leicester	Sir David Gill, K.C.B., F.R.S.	276	19
1908, Sept. 2	Dublin	Dr. Francis Darwin, F.R.S.	294	24
1909, Aug. 25......	Winnipeg	Prof. Sir J. J. Thomson, F.R.S.	117	13
1910, Aug. 31	Sheffield................	Rev. Prof. T. G. Bonney, F.R.S.	293	26
1911, Aug. 30	Portsmouth	Prof. Sir W. Ramsay, K.C.B., F.R.S.	284	21
1912, Sept. 4	Dundee	Prof. E. A. Schäfer, F.R.S...............	288	14
1913, Sept. 10	Birmingham	Sir Oliver J. Lodge, F.R.S...............	376	40
1914, July-Sept....	Australia	Prof. W. Bateson, F.R.S.	172	13
1915, Sept. 7	Manchester	Prof. A. Schuster, F.R.S.	242	19
1916, Sept. 5	Newcastle-on-Tyne...	Sir Arthur Evans, F.R.S.	164	12
1917	(No Meeting)		—	—
1918	(No Meeting)		—	—
1919, Sept. 9	Bournemouth	Hon. Sir C. Parsons, K.C.B., F.R.S ...	235	47
1920, Aug. 24......	Cardiff	Prof. W. A. Herdman, C.B.E., F.R.S.	288	11
1921, Sept. 7	Edinburgh	Sir T. E. Thorpe, C.B., F.R.S.	336	9
1922, Sept. 6	Hull	Sir C. S. Sherrington, G.B.E., Pres. R.S.	228	13

¶ Including 848 Members of the South African Association.
‡‡ Grants from the Caird Fund are not included in this and subsequent sums.

Annual Meetings—(continued).

Old Annual Members	New Annual Members	Associates	Ladies	Foreigners	Total	Amount received for Tickets	Sums paid on account of Grants for Scientific Purposes	Year
297	45	801	482	9	1915	£1801 0	£1072 10 0	1900
374	131	794	246	20	1912	2046 0	920 9 11	1901
314	86	647	305	6	1620	1644 0	947 0 0	1902
319	90	688	365	21	1754	1762 0	845 13 2	1903
449	113	1338	317	121	2789	2650 0	887 18 11	1904
937¶	411	430	181	16	2130	2422 0	928 2 2	1905
356	93	817	352	22	1972	1811 0	882 0 9	1906
339	61	659	251	42	1647	1561 0	757 12 10	1907
465	112	1166	222	14	2297	2317 0	1157 18 8	1908
290**	162	789	90	7	1468	1623 0	1014 9 9	1909
379	57	563	123	8	1449	1439 0	963 17 0	1910
349	61	414	81	31	1241	1176 0	922 0 0	1911
368	95	1292	359	88	2504	2349 0	845 7 6	1912
480	149	1287	291	20	2643	2756 0	978 17 1‡‡	1913
139	4160‖	539‖	—	21	5044‖	4873 0	1086 16 4	1914
287	116	628*	141	8	1441	1406 0	1159 2 8	1915
250	76	251*	73	—	826	821 0	715 18 10	1916
—	—	—	—	—	—	—	427 17 2	1917
—	—	—	—	—	—	—	220 13 3	1918
254	102	688*	153	3	1482	1736 0	160 0 0	1919

Old Annual Regular Members	Annual Members		Transferable Tickets	Students' Tickets					
	Meeting and Report	Meeting only							
136	192	571	42	120	20	1380	1272 10	959 13 9	1920
133	410	1394	121	343	22	2768	2599 15	418 1 10	1921
90	294	757	89	235‡	24	1730	1699 5	257 0 7	1922

** Including 137 Members of the American Association.

‖ Special arrangements were made for Members and Associates joining locally in Australia, see Report, 1914, p. 686. The numbers include 80 Members who joined in order to attend the Meeting of L'Association Française at Le Havre.

* Including Students' Tickets, 10*s*.

‡ Including Exhibitioners granted tickets without charge.

REPORT OF THE COUNCIL, 1921–22.

I. Professor Sir Ernest Rutherford, F.R.S., has been unanimously nominated by the Council to fill the office of President of the Association for the year 1923-24 (Liverpool Meeting).

II. The Council have to record their deep sense of the great generosity of the Hon. Sir Charles Parsons, K.C.B., F.R.S., ex-President, who has placed at their disposal a gift of £10,000 War Stock, for the general purposes of the Association.

Sir Charles Parsons also generously undertook to bear the cost of producing and publishing *The British Association: A Retrospect, 1831-1921*, which, at his suggestion, has been compiled by Mr. O. J. R. Howarth. A copy of this work has been graciously accepted by His Majesty the King, Patron of the Association.

The thanks of the Council have been conveyed to Mrs. Sidney Brown, for her gift of £75 as 'the John Perry's Guest Fund' for use by the General Treasurer in case of emergency connected with guests of the Association, any remainder to be at the disposal of the Council after five years (December 1926).

III. Resolutions referred by the General Committee, at the Edinburgh Meeting, to the Council for consideration and, if desirable, for action, were dealt with as follows:—

(*a*) The Council welcomed the General Committee's approval of their action in encouraging joint discussions between Sections.

(*b*) The Council made a standing order under which research committees are required to present their reports in duplicate fair copy by a date to be determined by the General Officers; one copy is retained for consideration by the General Officers, and the other forwarded on receipt to the Recorder of the Section concerned, who is desired, after consultation with the President of his Section, to inform the Secretary of the Association whether it is recommended to the General Officers that the report be set up in type in advance of the meeting. (Resolution of Section B.)

(*c*) The Council ascertained that the special powers conferred by them upon the Fuel Economy Committee were no longer required, and the Committee therefore assumed the ordinary position and powers of research committees. (Resolution of Section B.)

(*d*) The Council obtained from the Board of Education a statement relating to *Revised Regulations for Secondary Schools, England, 1921*, as follows:—

(1) The effect of Article 7 is to make it necessary that the course of work should be so arranged as to secure that every pupil who remains in the school till the age of 16 shall during his school life have passed through an adequate course of graduated instruction in each one of the subjects named in the Article.

(2) In a Circular issued in 1919 it was stated that Geography 'necessarily holds, as an essential part of all proper study of history, an important place in all courses belonging to Group B and Group C; and that the definition of Group C embodied in the current Regulations affords special opportunity for

increased attention to Geography in connection with the work in history.' This view is also applicable to the new Group D courses allowed under the recent Regulations.

(3) Geography is not accepted as a main subject in Group A (Science and Mathematics).

The groups B, C, D, referred to in (2) above refer to main subjects of study in advanced courses, and, as defined in the Regulations, consist respectively of '(B) Classics, viz., the civilisation of the ancient world as embodied in the language, literature, and history of Greece and Rome; (C) Modern Studies, viz., the language, literature and history of the countries of Western Europe in modern and mediæval times; (D) the civilisation (i) of Greece or Rome, and (ii) of England or another country of Western Europe in modern times, as embodied in their language, literature, and history.'

The correspondence embodying the above statement was published in the Press by order of the Council with the consent of the Board. (Resolution of Sections E and L.)

The Council, after further correspondence with the Board, were gratified to learn from the Draft *Regulations for Secondary Schools*, 1922, that the position of Geography in the curriculum was to be materially strengthened, and that it was to be included as a principal subject in Advanced Courses (Group E).

(*e*) The Council conveyed to the Census authorities of the United Kingdom a recommendation that the final census report should include the population not merely of municipal and other administrative areas, but also of urban aggregates. The recommendation was acknowledged by the Registrar-General. (Resolution of Section E.)

(*f*) The Council caused inquiry to be made as to the use of Mercator's projection for the international series of aeronautical maps. (Resolution of Section E.) Further discussion is anticipated in Section E at the Hull Meeting.

(*g*) The Council addressed universities, colleges, and a number of societies, chambers of commerce, etc., on the subject of the teaching of anthropology. A conference was then convened at Burlington House on May 23, 1922, and was attended by representatives of most of the bodies addressed, and a committee was nominated, and subsequently appointed by the Council, to confer with the Royal Anthropological Institute as to the possibility of its acting as a central institution for the encouragement of more general interest in anthropological studies, &c. (Resolution of Section H.)

Further consideration was delayed owing to the death of Dr. W. H. R. Rivers, President of the Institute, whom the Council deeply deplore both as a valued colleague and as President-designate of Section J for the Hull Meeting.

(*h*) The Council, on inquiry, found it unnecessary to proceed in the matter of a resolution of Section L on the position of music in the curriculum of secondary schools.

(*i*) The Council referred to the President of the Royal Society and, in his discretion, the Conjoint Board of Scientific Societies, the substance of resolutions by Section L and the Conference of Delegates on the high cost of postage of societies' publications.

IV. The Council took no action upon a resolution received from the Organising Committee of Section K (Botany) proposing that forestry should be regarded as included in the work of that Section, and a

resolution from the Organising Committee of Section M (Agriculture) in opposition to this proposal.

V. *Conference of Delegates and Corresponding Societies Committee.*—The Council approved the following report and memorandum:—

CORRESPONDING SOCIETIES COMMITTEE.

The Secretary of the Committee reported:—

I brought your letter of March 9 before a meeting of the Corresponding Societies Committee (at which there were present Mr. Sheppard (in the chair), Mr. Ashton, Dr. Bather, Sir George Fordham, Dr. Garson, Mr. Whitaker, the General Secretaries of the Association, and the Secretary of the Committee), and I have to report as follows:—

(1) That Mr. Ashton and I asked leave to withdraw our names as nominees for the Secretaryship and Presidency respectively of the Conference of Delegates at Hull, which was granted.

(2) That it was decided (*a*) that the suggestion with regard to payment by Corresponding Societies should be circulated in general terms among them all, so that it might be discussed at Hull, and (*b*) that the Council should be informed of this action. (Proposed by Professor Myres and seconded by Dr. Garson.)

(3) That all matters as to the Conference of Delegates at Hull should be left to the Council, and that the Committee would endeavour to carry out any suggestions that the Council might be pleased to make. (Proposed from the Chair and carried unanimously.)

(4) That Mr. Sheppard announced that an Exhibition on the lines suggested at the Conference of Delegates at Edinburgh would be arranged at Hull, and it was left to the Council to determine whether this should come under the heading of the Conference or otherwise.

(*Signed*) WILFRED MARK WEBB.

MEMORANDUM BY THE GENERAL OFFICERS.

The matters referred to the Corresponding Societies Committee by Council were considered by the Committee at a meeting on March 17 (as reported above).

After correspondence with the Chairman of the Committee, and with his entire concurrence, the General Officers submit the following suggestions:—

1. That the Conference at Hull should consider, in the first place, what steps should be taken, in accordance with the recommendation of the Committee in 1883, to induce local societies to group themselves round local (*i.e.* district) sub-centres for the interchange of information and for the more economical publication of the results of research. Such groups have been formed already in some districts (*e.g.* the Yorkshire Naturalists' Union, the Lincolnshire Union, and the South Eastern Union; and it is understood that a similar union is projected for the Scottish societies).

2. That at Hull the Yorkshire Naturalists' Union should be invited to explain its own procedure; and that in subsequent years it should be an instruction to the Conference to elicit similar co-operation within the district where the meeting is held. In this way all principal regions of the country will be dealt with in time.

3. That in future, to ensure such action by the Conference, there should be a local chairman and a local committee to assist in preparing the programme; and that the Corresponding Societies Committee should consist of a comparatively small standing nucleus of members appointed by the Council and empowered to co-opt the local chairman and committee for the time being.

4. That at Hull the President of the Conference should be a well-known naturalist (not necessarily local) especially interested in efficient co-operation between local societies. The names of Sir Sidney Harmer and Mr. W. Whitaker, the outgoing Chairman of the Corresponding Societies Committee, have been suggested. It would not seem to be necessary on this occasion to expect from the President a formal address.

5. That the routine work of the Committee should be conducted in future by the Office under the direction of the General Officers; and the local secretarial

work of each conference by a local secretary appointed by the Council on the recommendation of the Local Committee.

6. That it be an instruction to the Corresponding Societies Committee to prepare a general survey of local scientific societies, including information as to existing federations and local unions, and as to the organisation of the Congress of Societies in union with the Society of Antiquaries, since many of these societies undertake work in physical and biological science also.

7. That the Committee and the Hull Conference be asked to consider whether the delegates sent to the Conference might be authorised to act as the local representatives of the British Association in their respective districts.

8. That the suggestion made by the Corresponding Societies Committee as to a levy or subscription from the Societies be postponed until the Council has received a report from the Conference and considered in what respects the advantages derived by the Societies from their connection with the Association may be increased, or better understood, as for example in regard to improved facilities for publication, and to obtaining lecturers of recognised scientific standing.

9. That in addition to its work for the local societies, the Corresponding Societies Committee be asked to enter into correspondence with the principal societies concerned with special departments of science, so as to ensure that the British Association is in full touch with the more general needs of scientific workers throughout the country and in the Dominions. Some of the principal societies in the Dominions are already enrolled in this way, and have sent delegates from time to time to the Conferences.

Mr. W. Whitaker has been nominated as President of the Conference at the Hull Meeting.

The Corresponding Societies Committee has been nominated as follows: The President of the Association (*Chairman ex-officio*), Mr. T. Sheppard (*Vice-Chairman*), the General Secretaries, the General Treasurer, Dr. F. A. Bather, Mr. O. G. S. Crawford, Prof. P. F. Kendall, Mr. Mark L. Sykes, Dr. C. Tierney, Prof. W. W. Watts, Mr. W. Whitaker; with authority to co-opt representatives of the Scientific Societies of Liverpool and District.

VI. The Council have received reports from the General Treasurer throughout the year. His accounts have been audited, and are presented to the General Committee.

The Council made the following grants to research committees from the Caird Fund, additional to those approved by the General Committee at the Edinburgh Meeting:—

Annual Tables of Constants	£40
Inheritance of Colour in Lepidoptera	£25
Naples Zoological Station	£100

The Council amended the condition attached to the grant to £30 to the Stone Circles Committee, by resolving that the grant should be available if the excavations at Avebury were filled up under the direction of the Committee not later than May 15, 1922.

The second grant of £250 from the Caird Gift for research in radio-activity (for the year ending March 24, 1923) has been made to Sir Ernest Rutherford.

The Council decided to establish a series of 'British Association Exhibitions' for attendance at the Hull Meeting, offered to students, not above the standing of B.Sc., nominated by the senate of each of twenty universities and university colleges, and covering the railway fares of such students and their membership if not already regular

members. The Local Executive Committee at Hull kindly supplemented the above proposal by an offer of financial support and hospitality for nominees.

The Council resolved that life compositions received on and after January 1, 1922, shall be treated as capital and invested; subject that on the death of any member whose life composition has been thus placed to capital account, the amount of that composition shall be brought into the income account of the year.

The Council have undertaken, under a suitable agreement, to pay the major proportion of the premiums for an endowment policy on the life of Mr. O. J. R. Howarth, in order to provide him with a capital sum by way of pension at age sixty-five, or his dependants with the same in the event of his earlier death, so long as he remains in the service of the Association.

VII. The thanks of the Council have been conveyed to Miss A. Ashley, Miss L. Grier, and Mr. A. H. Gibson for their work in drawing up and preparing for publication reports on British Finance and British Labour (edited by Prof. A. W. Kirkaldy, and published by Messrs. Pitman).

The Council have instituted a new series of British Association reprints of selected communications, in standard paper covers.

The Council have decided to admit advertisements into the publications of the Association, and arrangements have been made with an advertising agency to this end.

VIII. The retiring Ordinary Members of the Council are:—

By seniority: Prof. W. A. Bone, Dr. A. Smith Woodward, Prof. W. R. Scott.

By least attendance: Sir R. Hadfield, Prof. J. Stanley Gardiner.

The Council nominated the following new members:—

Rt. Hon. Lord Bledisloe, Dr. W. E. Hoyle, Mr. A. G. Tansley,

leaving two vacancies to be filled by the General Committee without nomination by the Council.

A further vacancy is created by the lamented death of Dr. W. H. R. Rivers, to which reference has already been made.

The full list of nominations of Ordinary Members is as follows:—

Dr. E. F. Armstrong.	Sir A. Keith.
Dr. F. W. Aston.	Sir J. Scott Keltie.
Mr. J. Barcroft.	Professor A. W. Kirkaldy.
Rt. Hon. Lord Bledisloe.	Dr. P. Chalmers Mitchell.
Professor H. J. Fleure.	Sir J. E. Petavel.
Professor A. Fowler.	Sir W. J. Pope.
Sir R. A. Gregory.	Professor A. W. Porter.
Sir Daniel Hall.	Professor A. C. Seward.
Sir S. F. Harmer.	Sir Aubrey Strahan.
Dr. W. E. Hoyle.	Mr. A. G. Tansley.
Mr. J. H. Jeans.	Mr. W. Whitaker.

IX. The General Officers have been nominated by the Council as follows:—

General Treasurer, Dr. E. H. Griffiths.

General Secretaries, Prof. J. L. Myres and Mr. F. E. Smith.

The Council received with great regret Prof. H. H. Turner's intimation that he would not be able to attend a Meeting in Canada in 1924. Prof. Turner himself pointed out that it was desirable, on various grounds, that his successor should have experience of the working of an Annual Meeting at home before taking part in one overseas, and he therefore placed his office at the disposal of the General Committee as from the Hull Meeting. The Council are fortunate in securing the consent of Mr. F. E. Smith, Director of Scientific Research at the Admiralty, to nomination as Prof. Turner's successor. The Council and the Association owe a deep debt of gratitude to Prof. Turner for his unremitting care for the interests of the Association as General Secretary since 1913, and therefore during a time of exceptional difficulty, including as it has the Australian Meeting, the War, the revival of the annual meetings since the War, and the period when on the death of the late General Treasurer and Assistant Treasurer in 1920 he acted for some months as Treasurer in addition to his other work.

X. Dr. E. H. Griffiths and Prof. J. L. Myres have continued to act as representatives of the Association on the Conjoint Board of Scientific Societies.

XI. The following have been admitted as members of the General Committee:—

Dr. R. N. Rudmose Brown.	Dr. A. Lauder.
Dr. J. B. Firth.	Prof. P. Marshall.
Mr. C. T. Gimingham.	Prof. W. H. Pearsall.
Mr. Wilfred Hall.	Prof. H. C Plummer.
Dr. J. W. Heslop-Harrison.	Mr. F. E. Smith.
Dr. H. S. Holden.	Dr. T. W. Woodhead.

XII. The Council have authorised Mr. O. J. R. Howarth to use the title of Secretary of the Association, in lieu of Assistant Secretary, as pertaining to his office; and they recommend the amendment of the Rules accordingly wherever the latter title occurs (Chh. II., 2: III., 1; IV., 3; VI., 5; VIII., 2; IX., 5; XI., 2, 3).

BRITISH ASSOCIATION EXHIBITIONS.

The British Association Exhibitions, referred to in § VI. of the above report, were awarded to eighteen students nominated by the same number of universities and colleges, whose travelling expenses (railway fares) were met by the Association, which also issued complimentary students' tickets of membership to them; they were entertained in Hull by the Local Executive Committee. Six of the universities or colleges allowed travelling expenses for eight additional exhibitioners, who also received the other facilities indicated above. The exhibitioners were enabled to meet the President and general officers. One of their number (Mr. D. C. Ellis, of Loughborough College) was elected secretary for the purpose of communication by the exhibitioners as a body with the general officers and the Press.

GENERAL MEETINGS AT HULL.

Inaugural General Meeting.

On Wednesday, September 6, at 8.30 p.m., in the City Hall, Sir T. Edward Thorpe, C.B., F.R.S., resigned the office of President of the Association to Professor Sir C. S. Sherrington, G.B.E., Pres. R.S., who delivered an address on 'Some Aspects of Animal Mechanism' (for which see p. 1).

Evening Discourses.

On Friday, September 8, at 8.30 p.m., in the City Hall, Professor W. Garstang delivered a discourse on 'Fishing: Old Ways and New.'

On Tuesday, September 12, at 8.30 p.m., in the City Hall, Dr. F. W. Aston, F.R.S., delivered a discourse on 'The Atoms of Matter: their Size, Number, and Construction.'

Concluding General Meeting.

The concluding General Meeting was held in the Queen's Hall on Wednesday, September 13, at 12 noon, when, on the motion of the President, it was resolved by acclamation:—

'That the British Association do thank the City of Hull.'

PUBLIC LECTURES AT HULL.

Public or Citizens' Lectures were delivered as follows:—

Tuesday, September 5, at 8 p.m., in the City Hall: Dr. E. H. Griffiths, F.R.S., on 'The Conservation and Dissipation of Energy.'

Thursday, September 7, at 8 p.m., in the Royal Institution: Professor A. P. Coleman, F.R.S., on 'Labrador.'

Saturday, September 9, at 8 p.m., in the City Hall: Rev. A. L. Cortie, S.J., on 'The Earth's Magnetism.'

Monday, September 11, at 8 p.m., in the City Hall: Sir Westcott Abell, K.B.E., on 'The Story of the Ship.'

Tuesday, September 12, at 8 p.m., in the Royal Institution: Dr. A. Smith Woodward, F.R.S., on 'The Ancestry of Man.' (The accommodation available proved insufficient for the numbers who desired to hear this lecture, and many of the public, who were unable to obtain entrance, were admitted to Dr. Aston's evening discourse, which was being given on the same evening at the City Hall.)

Dr. Smith Woodward repeated his lecture to the citizens of Scarborough on Wednesday, September 13.

CHILDREN'S LECTURES AT HULL.

Special lectures for children were given in the Majestic Hall, George Street (through the courtesy of the Management), upon the following dates, at 10.30 a.m.:—

Thursday, September 7: Prof. J. Arthur Thomson, on 'Creatures of the Sea.'

Friday, September 8: Mr. F. Debenham, O.B.E., on 'The Antarctic.'

Tuesday, September 12: Prof. H. H. Turner, F.R.S., on 'The Telescope and what it tells us.'

GENERAL TREASURER'S ACCOUNT.

JULY 1, 1921, TO JUNE 30, 1922.

THE General Treasurer is able to present a more satisfactory report than was possible last year.

This is chiefly due to:—

(1) The great generosity of Sir Charles Parsons.

(2) The legacy of £450 from Mr. T. W. Backhouse.

(3) The success of the Edinburgh Meeting.

(4) The economies effected.

The appreciation in the capital value of our investments is also a matter for congratulation.

We cannot, however, count on a recurrence of such favourable events. It should be borne in mind that our administrative, printing, and other expenses are certain to increase as the activities of the Association are extended. I trust, therefore, that the favourable report now presented will not banish from the minds of our members the need for economy.

E. H. GRIFFITHS,
General Treasurer.

Balance Sheet,

LIABILITIES.

	£ s. d.	£ s. d.	£ s. d. Corresponding Figures, 1921.
To Sundry Creditors		246 15 1	282 3 7
,, Capital Account—			
General Fund per contra . . .	10,575 15 2		
Caird Fund do.	9,582 16 3		
Sir F. Bramwell's Gift for Inquiry into Prime Movers, 1931— £50 Consols accumulated to June 30, 1922, as per contra	53 14 6	20,212 5 11	*20,208 6 5*
,, Caird Fund Income and Expenditure Account—			
Balance at July 1, 1921	968 11 8		
Less Transferred to General Account in accordance with Council Minutes dated September 7, 1921 . .	257 0 7		
	711 11 1		
Less Excess of Expenditure over Income for Year to June 30, 1922 .	137 19 1	573 12 0	*968 11 8*
,, Caird Gift—			
Radio-Activity Investigation, Balance at July 1, 1921	1,008 8 8		
Add Dividends on Treasury Bonds .	38 10 0	1,046 18 8	*1,008 8 8*
,, Sir Charles Parsons' Gift		10,000 0 0	
,, John Perry Guest Fund— For cases of emergency connected with Guests of the Association . . .		75 0 0	
,, Life Compositions from January 1, 1922 .		45 0 0	
,, Legacy, T. W. Backhouse		450 0 0	
,, Income and Expenditure Account—			
Balance at July 1, 1921 . . .	2,054 19 2		
Add Excess of Income over Expenditure for the year . . .	567 0 1	2,621 19 3	*2,054 19 2*
		£35,271 10 11	*£24,522 9 6*

I have examined the foregoing Account with the Books and Vouchers, and certify the

Approved,

ARTHUR L. BOWLEY } *Auditors.*
A. W. KIRKALDY }

July 20, 1922.

June 30, 1922.

ASSETS.

		£ s. d.	£ s. d.	£ s. d. Corresponding Figures, 1921.
By Sundry Debtors			21 0 3	*259 2 5*
,, Investments on Capital Accounts—				
£4,651 10*s.* 5*d.* Consolidated 2½ per cent. Stock at cost		3,942 3 3		
£3,600 India 3 per cent. Stock at cost .		3,522 2 6		
£879 14*s.* 9*d.* £43 Great Indian Peninsula "B" Annuity at cost . . .		827 15 0		
£810 10*s.* 3*d.* £52 12*s.* 7*d.* War Stock, 1929/47 at cost		889 17 6		
£1,400 War Bonds 5 per cent. 1929/47, at cost		1,393 16 11	10,575 15 2	*10,575 15 2*
£6,794 8s. 4d. Value at date, £7,634 18*s.* 2*d.*				
,, Caird Fund—				
£2,627 0*s.* 10*d.* India 3½ per cent. Stock at cost		2,400 13 3		
£2,100 London and North Western Rly. Consolidated 4 per cent. Preference Stock at cost		2,190 4 3		
£2,500 Canada 3½ per cent. 1930/50 Registered Stock at cost . . .		2,397 1 6		
£2,500 London and South Western Rly. Consolidated 4 per cent. Preference Stock at cost		2,594 17 3	9,582 16 3	*9,582 16 3*
£5,889 18s. 2d. Value at date, £7,359 16*s.* 4*d.*				
,, Sir F. Bramwell's Gift—				
£50 2½ per cent Self-Cumulating Consolidated Stock as per last Balance Sheet	103 13 3	49 15 0		
Add accumulations to June 30, 1922 . .	7 17 10	3 19 6	53 14 6	*49 15 0*
	111 11 1			
By Caird Gift—				
£1,000 Registered Treasury Bonds, value at date, £1,105			1,000 0 0	*1,000 0 0*
,, Sir Charles Parsons' Gift—				
£10,000 5 per cent. War Loan, value at date, £10,025			10,000 0 0	
,, John Perry Guest Fund—				
£96 National Savings Certificates at cost			74 8 0	
,, Investments out of Income—				
£2,098 1*s.* 9*d.* Consolidated 2½ per cent. Stock at cost		1,200 0 0		
£1,500 Registered Treasury Bonds at cost, value at date, £2,857 10*s.* . . .		1,482 0 0	2,682 0 0	*900 0 0*
,, Cash—				
On Deposit		657 18 1		
At Bank		619 7 0		
In hand		4 11 8	1,281 16 9	*2,155 0 8*
Viz.:—				
Caird Fund . .	573 12 0			
Caird Gift . .	46 18 8			
John Perry Guest Fund (Balance) . .	0 12 0			
Life Compositions .	45 0 0			
Legacy, T. W. Backhouse . . .	450 0 0			
General Purposes .	165 14 1			
	£1,281 16 9			
			£35,271 10 11	*£24,522 9 6*

same to be correct. I have also verified the balances at the Bankers and the Investments

W. B. KEEN,
Chartered Accountant

Income and

FOR THE YEAR ENDED

EXPENDITURE.

	£ s. d.	£ s. d.	£ s. d. Corresponding Period, June 30, 1921.
To Heat and Lighting	10 17 3		*7 13 7*
,, Stationery	27 12 9		*43 13 11*
,, *Advertising*			*21 13 3*
,, Rent	8 5 0		*8 2 6*
,, Electric Light Installation	45 5 5(1)		*96 9 6*
,, Postages	97 1 1		*53 12 5*
,, Refund *re* Australian Meeting, 1914	75 0 0		
,, *Gift to Miss Stewardson*			*100 0 0*
,, Travelling Expenses	50 6 5(2)		*28 5 11*
,, Recorders and Secretaries' Travelling Expenses and Postages	331 4 7(2)		*121 3 4*
,, General Expenses	124 17 3		*171 5 2*
	770 19 9		
,, Salaries	998 13 4		*972 10 0*
,, Pension Fund	75 0 0		
,, Printing, Binding, etc.	1,974 13 8(3)		*1,475 10 11*
		3,819 6 9	
,, Miss Stewardson, as per contra		15 0 0	
,, Grants to Research Committees, etc.—			
Stress Committee	5 5 0		
Bronze Implements Committee	50 0 0		
Citizenship Committee	10 0 0		
Parthenogenesis Committee	5 0 0		
Colloid Chemistry Committee	5 0 0		
Mathematical Tables Committee	5 17 6		
Conjoint Board	10 0 0		
Zoology Organisation Committee	11 1 7		
Corresponding Societies Committee	40 0 0		
Credit Currency, etc., Committee	25 0 0		
Stone Circles Committee	30 0 0		
Kiltorcan Committee	15 0 0		
Malta Committee	25 0 0		
Fuel Economy Committee	5 0 0		
International Language Committee	5 0 0		
Oenothera Committee	4 16 6		
Gilbert White Memorial Committee	5 0 0		
		257 0 7	*418 1 10*
,, Balance being excess of Income over Expenditure for the year		567 0 1	
		£4,658 7 5	*£3,518 2 4*

(1) The electric lighting installation is complete, and the item will not recur.
(2) The greater distance of Edinburgh than of Cardiff from the homes of most of the Secretaries accounts for the increase.
(3) The increase is accounted for by the larger issue of the Annual Report consequent upon the larger attendance of members at Edinburgh, by the increased circulation of the 'Advancement of Science,' and by the printing of the new series of 'British Association Reprints.'

Caird

	£ s. d.	£ s. d.	£ s. d.
To Grants paid—			
Marine Biological Association	200 0 0		
Seismology Committee	100 0 0		
Table of Constants Committee	40 0 0		
Naples Table Committee	100 0 0		
Bronze Implements Committee (additional grant)	50 0 0		
Lepidoptera Committee	25 0 0		
		515 0 0	*100 0 0*
,, *Balance being excess of Income over Expenditure*			*275 19 0*
		£515 0 0	*£375 19 0*

Expenditure Account

JUNE 30, 1922.

INCOME.

	£	s.	d.	£	s.	d.	£ s. d. Corresponding Period, June 30, 1921.		
By Life Compositions to December 31, 1921				225	0	0	*150*	*0*	*0*
,, Annual Members' Subscriptions, Regular (Including £75 in advance, 1922/23 and £2 ,, 1923/24)				323	0	0	*367*	*0*	*0*
,, Annual Members' Subscriptions, Temporary (Including £64 in advance, 1922/23 and £1 ,, 1923/24)				1,364	0	0	*613*	*0*	*0*
,, Annual Members' Subscriptions, with Report (Including £60 in advance, 1922/23)				514	10	0	*371*	*10*	*0*
,, Transferable Tickets (Including £3 15s. in advance, 1922/23)				154	0	0	*47*	*10*	*0*
,, Students' Tickets				171	10	0	*60*	*0*	*0*
,, Life Members' Additional Subscriptions				17	2	0(4)	*282*	*4*	*0*
,, *Refund of Travelling Expenses re Australian Meeting, 1914*							*75*	*0*	*0*
,, Donations				5	0	0	*148*	*13*	*5*
,, ,, (Miss Stewardson), as per contra				15	0	0			
,, Interest on Deposits				78	14	4(5)	*103*	*11*	*7*
,, Sales of Publications (including £100 Royalties)				734	6	0(6)	*466*	*11*	*8*
,, Transfer from Caird Fund to meet grants as per contra				257	0	7			
,, Unexpended Balance of grants returned				98	12	7	*224*	*19*	*3*
,, Income Tax recovered				95	8	2	*95*	*11*	*0*
,, Dividends :—									
Consols	81	8	0				*81*	*8*	*0*
India 3 per cent.	75	12	0				*75*	*12*	*0*
Great Indian Peninsula "B" Annuity	23	13	3				*23*	*9*	*11*
War Stock	93	18	0				*92*	*3*	*0*
,, ,, (Sir Charles Parsons' Gift)	250	0	0						
Treasury Bonds	50	12	6				*6*	*0*	*9*
				575	3	9			
,, *Legacy*							*154*	*4*	*0*
,, *By Balance being excess of Expenditure over Income*							*80*	*2*	*9*
				£4,658	7	5	*£3,518*	*2*	*4*

(4) This figure represents the (probably) final response to the late General Treasurer's appeal to Old Life Members in 1919-20.

(5) On the reduction of the Bank Rate and of interest upon deposits, the sums held on deposit have been reduced in favour of investment.

(6) The royalties were paid in advance upon Messrs. Pitman's publications, 'British Finance' and 'British Labour.' The increase apart from this item is due mainly to sales of 'The Advancement of Science' and the 'British Association Reprints.'

Fund.

	£	s.	d.	£	s.	d.	£	s.	d.
By Dividends on Investments :—									
India 3½ per cent.	64	7	4						
Canada 3½ per cent. (including extra ½ per cent.)	71	2	3						
London & South Western Railway Consolidated 4 per cent. Preference Stock	70	0	0						
London & North Western Railway Consolidated 4 per cent. Preference Stock	58	16	0						
				264	5	7	*263*	*3*	*4*
,, Income Tax recovered				112	15	4	*112*	*15*	*8*
,, Balance being excess of Expenditure over Income for the year				137	19	1			
				£515	0	0	*£375*	*19*	*0*

RESEARCH COMMITTEES, Etc.

APPOINTED BY THE GENERAL COMMITTEE, MEETING IN HULL: SEPTEMBER, 1922.

Grants of money, if any, from the Association for expenses connected with researches are indicated in heavy type.

For Committees concerned with the Nucleus Catalogue for the Carnegie United Kingdom Trust, see end of this list.

SECTION A.—MATHEMATICS AND PHYSICS.

Seismological Investigations.—Prof. H. H. Turner (*Chairman*), Mr. J. J. Shaw (*Secretary*), Mr. C. Vernon Boys, Dr. J. E. Crombie, Sir H. Darwin, Dr. C. Davison, Sir F. W. Dyson, Sir R. T. Glazebrook, Prof. C. G. Knott, Prof. H. Lamb, Sir J. Larmor, Prof. A. E. H. Love, Prof. H. M. Macdonald, Prof. H. C. Plummer, Mr. W. E. Plummer, Prof. R. A. Sampson, Sir A. Schuster, Sir Napier Shaw, Dr. G. T. Walker. **£100** (Caird Fund grant).

To assist work on the Tides.—Prof. H. Lamb (*Chairman*), Dr. A. T. Doodson (*Secretary*), Colonel Sir C. F. Close, Dr. P. H. Cowell, Sir H. Darwin, Dr. G. H. Fowler, Admiral F. C. Learmonth, Prof. J. Proudman, Major G. I. Taylor, Prof. D'Arcy W. Thompson, Sir J. J. Thomson, Prof. H. H. Turner. **£25** (for printing).

Annual Tables of Constants and Numerical Data, chemical, physical, and technological. —Sir E. Rutherford (*Chairman*), Prof. A. W. Porter (*Secretary*), Mr. A. E. G. Egerton. **£40** from Caird Fund, to be applied for from Council.

Calculation of Mathematical Tables.—Prof. J. W. Nicholson (*Chairman*), Dr. J. R. Airey (*Secretary*), Mr. T. W. Chaundy, Prof. L. N. G. Filon, Prof. E. W. Hobson, Mr. G. Kennedy, and Profs. Alfred Lodge, A. E. H. Love, H. M. Macdonald, G. B. Mathews, G. N. Watson, and A. G. Webster. **£20** (for printing).

Determination of Gravity at Sea.—Prof. A. E. H. Love (*Chairman*), Dr. W. G. Duffield (*Secretary*), Mr. T. W. Chaundy, Sir H. Darwin, Prof. A. S. Eddington, Major E. O. Henrici, Sir A. Schuster, Prof. H. H. Turner.

Investigation of the Upper Atmosphere.—Sir Napier Shaw (*Chairman*), Mr. C. J. P. Cave (*Secretary*), Prof. S. Chapman, Mr. J. S. Dines, Mr. W. H. Dines, Sir R. T. Glazebrook, Col. E. Gold, Dr. H. Jeffreys, Sir J. Larmor, Mr. R. G. K. Lempfert, Prof. F. A. Lindemann, Dr. W. Makower, Sir J. E. Petavel, Sir A. Schuster, Dr. G. C. Simpson, Mr. F. J. W. Whipple, Prof. H. H. Turner.

To aid the work of Establishing a Solar Observatory in Australia.—Prof. H. H. Turner (*Chairman*), Dr. W. G. Duffield (*Secretary*), Rev. A. L. Cortie, Dr. W. J. S. Lockyer, Mr. F. McClean, Sir A. Schuster.

SECTION B.—CHEMISTRY.

Colloid Chemistry and its Industrial Applications.—Prof. F. G. Donnan (*Chairman*), Dr. W. Clayton (*Secretary*), Mr. E. Ardern, Dr. E. F. Armstrong, Prof. Sir W. M. Bayliss, Prof. C. H. Desch, Dr. A. E. Dunstan, Mr. H. W. Greenwood, Mr. W. Harrison, Mr. E. Hatschek, Mr. G. King, Prof. W. C. McC. Lewis, Prof. J. W. McBain, Dr. R. S. Morell, Profs. H. R. Proctor and W. Ramsden, Sir E. J. Russell, Mr. A. B. Searle, Dr. S. A. Shorter, Dr. R. E. Slade, Mr. Sproxton, Dr. H. P. Stevens, Mr. H. B. Stocks, Mr. R. Whymper. **£5.**

Absorption Spectra and Chemical Constitution of Organic Compounds.—Prof. I. M. Heilbron (*Chairman*), Prof. E. E. C. Baly (*Secretary*), Prof. A. W. Stewart. **£10.**

SECTION C.—GEOLOGY.

The Old Red Sandstone Rocks of Kiltorcan, Ireland.—Prof. Grenville Cole (*Chairman*), Prof. T. Johnson (*Secretary*), Dr. J. W. Evans, Dr. R. Kidston, Dr. A. Smith Woodward. **£15.**

To excavate Critical Sections in the Palæozoic Rocks of England and Wales.—Prof. W. W. Watts (*Chairman*), Prof. W. G. Fearnsides (*Secretary*), Prof. W. S. Boulton, Mr. E. S. Cobbold, Prof. E. J. Garwood, Mr. V. C. Illing, Dr. J. E. Marr, Dr. W. K. Spencer.

The Collection, Preservation, and Systematic Registration of Photographs of Geological Interest.—Prof. E. J. Garwood (*Chairman*), Prof. S. H. Reynolds (*Secretary*), Mr. G. Bingley, Dr. T. G. Bonney, Messrs. C. V. Crook, R. Kidston, and A. S. Reid, Sir J. J. H. Teall, Prof. W. W. Watts, and Messrs. R. Welch and W. Whitaker.

To consider the preparation of a List of Characteristic Fossils.—Prof. P. F. Kendall (*Chairman*), Mr. H. C. Versey (*Secretary*), Prof. W. S. Boulton, Dr. A. R. Dwerryhouse, Profs. J. W. Gregory, Sir T. H. Holland, and S. H. Reynolds, Dr. Marie C. Stopes, Dr. J. E. Marr, Prof. W. W. Watts, Mr. H. Woods, and Dr. A. Smith Woodward. **£5.**

To investigate the Flora of Lower Carboniferous times as exemplified at a newly discovered locality at Gullane, Haddingtonshire.—Dr. R. Kidston (*Chairman*), Prof. W. T. Gordon (*Secretary*), Dr. J. S. Flett, Prof. E. J. Garwood, Dr. J. Horne, and Dr. B. N. Peach.

To investigate the Stratigraphical Sequence and Palæontology of the Old Red Sandstone of the Bristol district.—Dr. H. Bolton (*Chairman*), Mr. F. S. Wallis (*Secretary*), Miss Edith Bolton, Mr. D. E. I. Innes, Prof. C. Lloyd Morgan, Prof. S. H. Reynolds. **£15.**

SECTION D.—ZOOLOGY

To aid competent Investigators selected by the Committee to carry on definite pieces of work at the Zoological Station at Naples.—Prof. E. S. Goodrich (*Chairman*), Prof. J. H. Ashworth (*Secretary*), Dr. G. P. Bidder, Prof. F. O. Bower, Dr. W. B. Hardy, Sir S. F. Harmer, Prof. S. J. Hickson, Sir E. Ray Lankester, Prof. W. C. McIntosh. **£100** from Caird Fund, subject to approval of Council.

To summon meetings in London or elsewhere for the consideration of matters affecting the interests of Zoology, and to obtain by correspondence the opinion of Zoologists on matters of a similar kind, with power to raise by subscription from each Zoologist a sum of money for defraying current expenses of the organisation.—Prof. S. J. Hickson (*Chairman*), Mr. R. A. Wardle (*Secretary*), Profs. G. C. Bourne, A. Dendy, J. Stanley Gardiner, W. Garstang, Marcus Hartog, Sir W. A. Herdman, J. Graham Kerr, R. D. Laurie, E. W. MacBride, A. Meek, Dr. P. Chalmers Mitchell, Prof. E. B. Poulton, Prof. W. M. Tattersall.

Zoological Bibliography and Publication.—Prof. E. B. Poulton (*Chairman*), Dr. F. A. Bather (*Secretary*), Mr. E. Heron-Allen, Dr. W. E. Hoyle, Dr. P. Chalmers Mitchell. **£1.**

Parthenogenesis.—Prof. A. Meek (*Chairman*), Mr. A. D. Peacock (*Secretary*), Mr. R. S. Bagnall, Dr. J. W. Heslop-Harrison. **£5.**

To nominate competent Naturalists to perform definite pieces of work at the Marine Laboratory, Plymouth.—Prof. A. Dendy (*Chairman and Secretary*), Prof. E. S. Goodrich, Prof. J. P. Hill, Prof. S. J. Hickson, Sir E. Ray Lankester.

Experiments in Inheritance in Silkworms.—Prof. W. Bateson (*Chairman*), Mrs. Merritt Hawkes (*Secretary*), Dr. F. A. Dixey, Prof. E. B. Poulton, Prof. R. C. Punnett.

Experiments in Inheritance of Colour in Lepidoptera.—Prof. W. Bateson (*Chairman and Secretary*), Dr. F. A. Dixey, Prof. E. B. Poulton. **£2 17s.**

SECTION E.—GEOGRAPHY.

To consider the advisability of making a provisional Population Map of the British Isles, and to make recommendations as to the method of construction and reproduction.—Mr. H. O. Beckit (*Chairman*), Mr. F. Debenham (*Secretary*), Mr. J. Bartholomew, Prof. H. J. Fleure, Mr. R. H. Kinvig, Mr. A. G. Ogilvie, Mr. O. H. T. Rishbeth, Prof. P. M. Roxby.

SECTIONS E, L.—GEOGRAPHY, EDUCATION.

To formulate suggestions for a syllabus for the teaching of Geography both to Matriculation Standard and in Advanced Courses; to report upon the present position of the geographical training of teachers, and to make recommendations thereon: and to report, as occasion arises, to Council through the Organising Committee of Section E, upon the practical working of Regulations issued by the Board of Education affecting the position of Geography in Training Colleges and Secondary Schools.—Prof. T. P. Nunn (*Chairman*), Mr. W. H. Barker (*Secretary*), Mr. L. Brooks, Prof. H. J. Fleure, Mr. O. J. R. Howarth, Sir H. J. Mackinder, Prof. J. L. Myres, and Prof. J. F. Unstead (*from Section E*); Mr. Adlam, Mr. D. Berridge, Mr. C. E. Browne, Sir R. Gregory, Mr. E. Sharwood Smith, Mr. E. R. Thomas, Miss O. Wright (*from Section L*). **£10.**

SECTION G.—ENGINEERING.

To report on certain of the more complex Stress Distributions in Engineering Materials. —Prof. E. G. Coker (*Chairman*), Prof. L. N. G. Filon and Prof. A. Robertson (*Secretaries*), Prof. A. Barr, Dr. Gilbert Cook, Prof. W. E. Dalby, Sir J. A. Ewing, Messrs. A. R. Fulton and J. J. Guest, Dr. B. P. Haigh, Profs. Sir J. B. Henderson, C. E. Inglis, F. C. Lea, A. E. H. Love, and W. Mason, Sir J. E. Petavel, Dr. F. Rogers, Dr. W. A. Scoble, Mr. R. V. Southwell, Dr. T. E. Stanton, Mr. C. E. Stromeyer, Mr. J. S. Wilson. **£25.**

SECTION H.—ANTHROPOLOGY.

To report on the Distribution of Bronze Age Implements.—Prof. J. L. Myres (*Chairman*), Mr. H. Peake (*Secretary*), Dr. E. C. R. Armstrong, Mr. Leslie Armstrong, Dr. G. A. Auden, Mr. H. Balfour, Mr. L. H. D. Buxton, Mr. O. G. S. Crawford, Sir W. Boyd Dawkins, Prof. H. J. Fleure, Mr. G. A. Garfitt, Prof. Sir W. Ridgeway. **£100** (£20 from general funds; £80 from Caird Fund to be applied for from Council.)

To conduct Archæological Investigations in Malta.—Prof. J. L. Myres (*Chairman*), Sir A. Keith (*Secretary*), Dr. T. Ashby, Mr. H. Balfour, Dr. R. R. Marett, Mr. H. Peake. **£25.**

To conduct Explorations with the object of ascertaining the Age of Stone Circles.—Sir C. H. Read (*Chairman*), Mr. H. Balfour (*Secretary*), Dr. G. A. Auden, Prof. Sir W. Ridgeway, Dr. J. G. Garson, Sir Arthur Evans, Sir W. Boyd Dawkins, Prof. J. L. Myres, Mr. H. J. E. Peake.

To excavate Early Sites in Macedonia.—Prof. Sir W. Ridgeway (*Chairman*), Mr S. Casson (*Secretary*), Prof. R. C. Bosanquet, Dr. W. L. H. Duckworth, Prof. J. L. Myres.

To report on the Classification and Distribution of Rude Stone Monuments.—Dr R. R. Marett (*Chairman*), Prof. H. J. Fleure (*Secretary*), Mr. O. G. S. Crawford, Miss R. M. Fleming, Mr. G. Marshall, Prof. J. L. Myres, Mr. H. J. E. Peake. **£5.**

The Collection, Preservation, and Systematic Registration of Photographs of Anthropological Interest.—Sir C. H. Read (*Chairman*), Mr. E. N. Fallaize (*Secretary*), Dr. G. A. Auden, Dr. H. O. Forbes, Mr. E. Heawood, Prof. J. L. Myres, Mr. E. Torday.

To conduct Archæological and Ethnological Researches in Crete.—Dr. D. G. Hogarth (*Chairman*), Prof. J. L. Myres (*Secretary*), Prof. R. C. Bosanquet, Dr. W. L. H. Duckworth, Sir A. Evans, Prof. Sir W. Ridgeway, Dr. F. C. Shrubsall.

To co-operate with Local Committees in excavation on Roman Sites in Britain.—Prof. Sir W. Ridgeway (*Chairman*), Mr. H. J. E. Peake (*Secretary*), Dr. T. Ashby, Mr. Willoughby Gardner, Prof. J. L. Myres.

To report on the present state of knowledge of the Ethnography and Anthropology of the Near and Middle East.—Dr. A. C. Haddon (*Chairman*), Mr. E. N. Fallaize (*Secretary*), Mr. S. Casson (*Secretary*), Prof. H. J. Fleure, Mr. H. J. E. Peake. **£10.**

To report on the present state of knowledge of the relation of early Palæolithic Implements to Glacial Deposits.—Mr. H. J. E. Peake (*Chairman*), Mr. E. N. Fallaize (*Secretary*), Mr. H. Balfour, Mr. M. Burkitt.

To investigate the Lake Villages in the neighbourhood of Glastonbury in connection with a Committee of the Somerset Archæological and Natural History Society.—Sir W. Boyd Dawkins (*Chairman*), Mr. Willoughby Gardner (*Secretary*), Mr. H. Balfour, Mr. A. Bulleid, Mr. F. S. Palmer, Mr. H. J. E. Peake.

To co-operate with a Committee of the Royal Anthropological Institute in the exploration of Caves in the Derbyshire district.—Sir W. Boyd Dawkins (*Chairman*), Mr. G. A. Garfitt (*Secretary*), Mr. Leslie Armstrong, Mr. E. N. Fallaize, Dr. R. R. Marett, Mr. H. J. E. Peake, Prof. W. M. Tattersall. **£25.**

To investigate processes of Growth in Children, with a view to discovering Differences due to Race and Sex, and further to study Racial Differences in Women.—Sir A. Keith (*Chairman*), Prof. H. J. Fleure (*Secretary*), Dr. A. Low, Prof. F. G. Parsons, Dr. F. C. Shrubsall. **£20.** (A proportion not exceeding one-half of this grant may be expended on railway fares incurred in course of the investigation.)

To conduct Excavations and prepare a Survey of the Coldrum Megalithic Monument.—Sir A. Keith (*Chairman*), Prof. H. J. Fleure (*Secretary*), Mr. O. G. S. Crawford, Mr. H. J. E. Peake. **£20.**

To report on the existence and distribution of Megalithic Monuments in the Isle of Man.—Prof. H. J. Fleure (*Chairman*), Dr. Cyril Fox (*Secretary*), Mr. O. G. S. Crawford, Sir W. Herdman, Mr. P. M. C. Kermode, Rev. Canon Quine.

To report on proposals for an Anthropological and Archæological Bibliography, with power to co-operate with other bodies.—Dr. A. C. Haddon (*Chairman*), Mr. E. N. Fallaize (*Secretary*), Dr. T. Ashby, Mr. W. H. Barker, Mr. O. G. S. Crawford, Prof. H. J. Fleure, Prof. J. L. Myres, Mr. H. J. E. Peake, Dr. D. Randall-MacIver, Mr. T. Sheppard.

To report on the best means of publishing a monograph by Dr. Fox on the Archæology of the Cambridge Region.—Dr. A. C. Haddon (*Chairman*), Mr. H. J. E. Peake (*Secretary*), Prof. H. J. Fleure, Prof. J. L. Myres.

To report on the progress of Anthropological Teaching in the present century—Dr. A. C. Haddon (*Chairman*), Prof. J. L. Myres (*Secretary*), Prof. H. J. Fleure, Dr. R. R. Marett, Prof. C. G. Seligman.

SECTION I.—PHYSIOLOGY.

Efficiency of Movement in Men equipped with Artificial Limbs.—Prof. E. P. Cathcart (*Chairman*), Prof. A. V. Hill (*Secretary*), Dr. Hort. **£20.**

Muscular Stiffness in relation to Respiration.—Prof. A. V. Hill (*Chairman*), Dr. Ff. Roberts (*Secretary*), Mr. J. Barcroft. **£15.**

SECTION J.—PSYCHOLOGY.

The Place of Psychology in the Medical Curriculum.—Prof. G. Robertson (*Chairman*), Dr. W. Brown (*Secretary*), Dr. J. Drever, Dr. R. G. Gordon, Dr. C. S. Myers, Prof. T. H. Pear, Dr. F. C. Shrubsall.

Vocational Tests.—Dr. C. S. Myers (*Chairman*), Dr. G. H. Miles (*Secretary*), Mr. C. Burt, Prof. T. H. Pear, Mr. F. Watts, Dr. L. Wynn-Jones.

SECTION K.—BOTANY.

To continue Breeding Experiments on Oenothera and other Genera.—Dr. A. B. Rendle (*Chairman*), Dr. R. R. Gates (*Secretary*), Prof. W. Bateson, Mr. W. Brierley, Prof. O. V. Darbishire, Dr. M. C. Rayner. **£2. 8s. 6d.**

Primary Botanical Survey in Wales.—Dr. E. N. Miles Thomas (*Chairman*), Prof. O. V. Darbishire (*Secretary*), Miss A. J. Davey, Prof. McLean, Prof. F. W. Oliver, Prof. Stapledon, Mr. A. G. Tansley, Miss E. Vachell, Miss Wortham. **£10.** (A proportion not exceeding three-quarters of this grant may be expended on railway fares incurred in course of the investigation.)

SECTION L.—EDUCATIONAL SCIENCE.

Training in Citizenship.—Rt. Rev. J. E. C. Welldon (*Chairman*), Lady Shaw (*Secretary*), Mr. C. H. Blakiston, Mr. G. D. Dunkerley, Mr. W. D. Eggar, Mr. J. C. Maxwell Garnett, Sir R. A. Gregory, Mr. Spurley Hey, Miss E. P. Hughes, Sir T. Morison. **£50.**

To inquire into the Practicability of an International Auxiliary Language.—Dr. H. Foster Morley (*Chairman*), Dr. E. H. Tripp (*Secretary*), Mr. E. Bullough, Prof. J. J. Findlay, Sir Richard Gregory, Mr. W. B. Hardy, Dr. C. W. Kimmins, Sir E. Cooper Perry, Mr. Nowell Smith, Mr. A. E. Twentyman.

CORRESPONDING SOCIETIES.

Corresponding Societies Committee.—The President of the Association (*Chairman ex-officio*), Mr. T. Sheppard (*Vice-Chairman*), the General Secretaries, the General Treasurer, Dr. F. A. Bather, Mr. O. G. S. Crawford, Prof. P. F. Kendall, Mr. Mark L. Sykes, Dr. C. Tierney, Prof. W. W. Watts, Mr. W. Whitaker; with authority to co-opt representatives of the Scientific Societies of Liverpool and District. **£40** for preparation of bibliography and report.

LIST OF COMMITTEES

Appointed by the General Committee to co-operate with the Carnegie United Kingdom Trust in preparing the Scientific Sections of a Nucleus Catalogue of Books for the use of Rural Libraries.

Section B (Chemistry).—Prof. C. H. Desch (*Chairman*), Dr. A. Holt (*Secretary*), Dr. C. H. Keane.

Section C (Geology).—Dr. J. S. Flett (*Chairman*), Mr. W. Whitaker (*Secretary*), Dr. J. W. Evans.

Section D (Zoology).—Sir S. F. Harmer (*Chairman*), Dr. W. T. Calman (*Secretary*), Prof. J. H. Ashworth, Dr. P. Chalmers Mitchell.

Section E (Geography).—Dr. H. R. Mill (*Chairman*), Dr. R. N. Rudmose Brown (*Secretary*), Mr. G. G. Chisholm, Mr. O. J. R. Howarth, Dr. Marion Newbigin.

Section F (Economics).—Prof. E. Cannan (*Convener*), Prof. H. M. Hallsworth, Miss Jebb.

Section H (Anthropology).—Dr. E. S. Hartland (*Chairman*), Mr. E. N. Fallaize (*Secretary*), Mr. W. Crooke, Prof. H. J. Fleure.

Section I (Physiology).—Prof. H. E. Roaf (*Chairman*), Dr. C. Lovatt Evans (*Secretary*).

Section J (Psychology).—Dr. J. Drever (*Chairman*), Miss Bickersteth (*Secretary*), Dr. H. J. Watt.

Section K (Botany).—Dr. H. W. T. Wager (*Chairman*), Mr. F. T. Brooks (*Secretary*), Prof. W. Neilson Jones, Prof. F. E. Weiss.

Section L (Education).—Dr. A. Darroch (*Chairman*), Prof. T. P. Nunn.

THE CAIRD FUND.

An unconditional gift of £10,000 was made to the Association at the Dundee Meeting, 1912, by Mr. (afterwards Sir) J. K. Caird, LL.D., of Dundee.

The Council, in its report to the General Committee at the Birmingham Meeting, made certain recommendations as to the administration of this Fund. These recommendations were adopted, with the Report, by the General Committee at its meeting on September 10, 1913.

The following allocations have been made from the Fund by the Council to September 1922.

Naples Zoological Station Committee (p. xxvii).—£50 (1912–13); £100 (1913–14); £100 annually in future, subject to the adoption of the Committee's report (reduced to £50 during war; suspended, 1920–21, pending approval by Council of Committee's report on future control of the Station, etc.).

Seismology Committee (p. xxvi).—£100 (1913–14); £100 annually in future, subject to the adoption of the Committee's report.

Radiotelegraphic Committee (p. 273).—£500 (1913–14).

Magnetic Re-survey of the British Isles (in collaboration with the Royal Society).—£250.

Committee on Determination of Gravity at Sea (p. xxvi).—£100 (1914–15).

Annual Tables of Constants (p. xxvi).—£40.

Mr. F. Sargent, Bristol University, in connection with his Astronomical Work.—£10 (1914).

Organising Committee of Section F (Economics), towards expenses of an Inquiry into Outlets for Labour after the War.—£100 (1915).

Rev. T. E. R. Phillips, for aid in transplanting his private observatory.—£20 (1915).

Committee on Fuel Economy (p. 277).—£25 (1915–16), £10 (1919–20).

Committee on Training in Citizenship (p. xxx).—£10 (1919–20).

Geophysical Committee of Royal Astronomical Society.—£10 (1920).

Conjoint Board of Scientific Societies.—£10 (1920); £10 (1921).

Marine Biological Association, Plymouth.—£200 (1921).

In and since 1921, the Council have authorised expenditure from accumulated income of the fund upon grants to Research Committees approved by the General Committee by way of supplementing sums available from the general funds of the Association, and in addition to grants ordinarily made by, or applied for from, the Council.

Sir J. K. Caird, on September 10, 1913, made a further gift of £1,000 to the Association, to be devoted to the study of Radio-activity. In 1920 the Council decided to devote the principal and interest of this gift at the rate of £250 per annum for five years to purposes of the research intended. The grants for the year ending March 24, 1922 and 1923 were made to Sir E. Rutherford, F.R.S.

RESOLUTIONS & RECOMMENDATIONS.

The following Resolutions and Recommendations were referred to the Council (unless otherwise stated) by the General Committee at Hull for consideration and, if desirable, for action:—

From Section D.

That the Council be asked to support Dr. Potts in an application to the Committee on Sea Action of the Institution of Civil Engineers, for a grant in aid of his investigation upon the life-history of *Teredo.*

From Section E.

The Committee of Section E draws the attention of Council to the new Regulations for Secondary Schools issued by the Board of Education, and asks the Council to express to the Board its gratification at the inclusion of Group E (Advanced Courses) which allows Geography to be taken as an advanced study in combination with two other approved subjects.

(The General Committee instructed the General Secretaries to take immediate action on the above resolution.)

From Section G.

Although making no definite suggestion to the Council as to the method of procedure, the Committee of Section G would view with favour a scheme for raising a fund for the relief of distinguished aged scientists who are in need as the result of the present conditions on the Continent of Europe.

From Section L.

That Section L is cordially in favour of continuing the lectures for children, but hopes that in future such lectures may be arranged for the afternoons rather than the mornings in order that they may not clash with the meetings of the Sections.

(The General Committee considered it unnecessary to refer the above resolution to the Council, on receiving the General Officers' assurance that the matter would be borne in mind.)

From the Committee of Recommendations.

(*a*) That the Council be requested to consider the following resolution from the Museums Association :—That in the opinion of the Museums Association the time has arrived when it is desirable in the interest of the country to appoint a Royal Commission to investigate and report upon the work of the Museums of the United Kingdom in relation to industries and general culture.

(*b*) That if, in any application for a grant from the funds of the Association, any payment of travelling expenses (fares only) is contemplated, the amount to be so allocated must be stated in the application, and the payment of such expenses expressly sanctioned by the Committee of Recommendations and the General Committee, or, in the event of subsequent emergency, by the Council.

From the Conference of Delegates of Corresponding Societies.

(*a*) To invite the scientific societies of Liverpool and District, on the occasion of the British Association's visit in 1923, to consider what further provision, if any, is desirable for co-operation between them in the advancement of Science, as, for example, for scientific research, for the discussion of regional problems, and for the publication of results.

(*b*) To invite the Delegates sent to the Conference by the Corresponding Societies to render any assistance in their power in making known, in their respective districts, the objects and methods of the British Association, and to communicate to the Secretary of the Association the names and addresses of scientific workers and others to whom the preliminary programme of the next meeting should be sent.

(*c*) To call the attention of the Council to the inadequacy, discontinuity, and occasional overlap of scientific bibliographies already issued, and to request the Council to consider what steps may be taken, by the Association itself or otherwise, to make more systematic provision for the bibliography of the departments of science represented in the Sections of the Association.

(*d*) To request the Council to make known to the principal Government Departments, in any way which may seem desirable, the assistance which may be obtained by them through the local societies in scientific inquiries involving regional distributions.

(*e*) To call the attention of Scientific Societies to the necessity of retaining in all offprints from their publications the original numbering of the pages, and of providing full reference to the date, place, and title of the publication from which they are extracted.

(*f*) To call the attention of the Council to the value of the Regional Exhibit arranged for the Hull Meeting by the Yorkshire Naturalists' Union, and to suggest that it is desirable that such an exhibit should, if possible, be included regularly in the programme of the Annual Meeting.

(*g*) To inform the Conference of Delegates that the present practice of the Association is to present a copy of the Annual Report to each Society sending a Delegate to the Conference, recognising the practice by which one Delegate sometimes represents more than one Society, and to recommend that in future no Delegate be entitled to more than one copy, however many Societies he may represent; but that if any Society desires a copy of the Report it may be supplied at the reduced price of ten shillings.

The General Committee also formulated the following resolution:—

That in view of the material hardship imposed upon members attending the meetings of the British Association through the continuance of the unduly high railway charges, it is expedient that the Council should be requested to investigate the possibility of joint action being taken with other kindred associations with a view to obtaining a restoration of the customary travelling facilities and concessions allowed to such organisations before the war.

COMMUNICATIONS RECOMMENDED FOR PRINTING IN EXTENDED FORM.

Section A.—Extended abstract of Prof. P. Weiss's remarks in the Discussion on the Origin of Magnetism. (See p. 411.)

Section B.—Report of Discussion on the Nitrogen Industry. (See p. 415.)

Section L.—Lord Meston's Address on Imperial Citizenship. (See p. 423.)

THE PRESIDENTIAL ADDRESS.

SOME ASPECTS OF ANIMAL MECHANISM.

BY

Professor Sir C. S. SHERRINGTON, G.B.E., Sc.D., D.Sc., LL.D., Pres.R.S.,

PRESIDENT OF THE ASSOCIATION.

It is sometimes said that Science lives too much to itself. Once a year it tries to remove that reproach. The British Association meeting is that annual occasion, with its opportunity of talking in wider gatherings about scientific questions and findings. Often the answers are tentative. Commonly questions most difficult are those that can be quite briefly put. Thus, 'Is the living organism a machine?' 'Is life the running of a mechanism?' The answer cannot certainly be as short as the question. But let us, in the hour before us, examine some of the points it raises.

Of course for us the problem is not the why of the living organism but the how of its working. If we put before ourselves some aspects of this working we may judge for ourselves some at least of the contents of the question. It might be thought that the problem is presented at its simplest in the simplest forms of life. Yet it is in certain aspects more seizable in complex animals than it is in simpler forms. And so let us turn thither.

Our own body is full of exquisite mechanism. Many exemplifications could be chosen. There is the mechanism by which the general complex internal medium, the blood, is kept relatively constant in its chemical reaction, despite the variety of the food replenishing it and the fluctuating draft from and input into it from various organs and tissues. In this mechanism the kidney cells and the lung cells form two of the main sub-mechanisms. And one part of the latter is the delicate mechanism linking the condition of the air at the bottom of the lungs with that particular part of the nervous system which manages the ventilation of the lungs. On that ventilation depends the proper respiratory condition of the blood. The nervous centre which manages the rhythmic breathing of the chest is so responsive to the respiratory

state of the blood supplied to itself that, as shown by Drs. Haldane and Priestley some years ago, the very slightest increase in the partial pressure of carbon dioxide at the bottom of the lungs at once suitably increases the ventilation of the chest. And dovetailed in with this mechanism is a further one working for adjustment in the same direction. As the lung is stretched by each inbreath the respiratory condition of the nervous centre, already attuned to the respiratory quality of the air in the lungs, sets the degree to which inspiration shall fill them ere there ensue the opposite movement of outbreath. All this regulation, although the nervous system takes part in it, is a mechanism outside our consciousness. Part of it is operated chemically; part of it is reflex reaction to a stimulus of mechanical kind, though as such unperceived. The example taken has been nervous mechanism. If in the short time at disposal we confine our examples to the nervous system, to do so will have the advantage that in one respect that system presents our problem possibly at its fullest.

To turn, therefore, to another instance, mainly nervous. Muscles execute our movements; they also maintain our postures. This postural action of muscles is produced by nerve-centres which form a system more or less their own. One posture of great importance thus maintained is that of standing, the erect posture. This involves due co-operation of many separate muscles in many parts. Even in absence of those portions of the brain to which consciousness is adjunct the lower nerve-centres successfully bring about and maintain all this co-operation of muscles which results in the erect posture. For instance, the animal in this condition, if set on its feet, stands. It stands reflexly. More than that, it adjusts its standing posture to required conditions. If the pose of one of the limbs be shifted that shift induces a compensatory shift in the other limbs, so that stability is retained. A turn of the creature's neck sidewise and the body and limbs of themselves take up a fresh attitude appropriate to the side-turned head. Each particular pose of the neck telegraphs off to the limbs and body a particular posture required from them, and that posture is then maintained so long as the neck posture is maintained. Stoop the creature's neck and the forelimbs bend down as if to seek something on the floor. Tilt the muzzle upward and the forelimbs straighten and the hind limbs crouch as if to look up at a shelf. Purely reflex mechanism provides most kinds of ordinary postures.

Mere reflex action provides these harmonies of posture. The nerve-centres evoke for this purpose in the required muscles a mild, steady contraction, with tension largely independent of the muscle length and little susceptible to fatigue. Nerve-fibres run from muscle to nerve-centre. By these each change in tension or length of the muscle is reported to the activating nerve-centre. They say 'Tension rising, you

must slacken,' or conversely. There also play a part organs whose stimulation changes with change of their relation to the line of gravity. Thus, a pair of tiny water-filled bags set one in each side of the skull. In each of these a patch of cells endowed with a special nerve. Attached to hairlets of these cells a tiny crystalline stone whose pressure acts as a stimulus through them to the nerve. The nerve of each gravity-bag connects, through chains of nerve-centres, with the muscles of all the limbs and of one side of the neck. In the ordinary erect posture of the head the stimulation by the two bags right and left is equal, because the two gravity-stones then lie symmetrically. The result, then, is a symmetrical muscular effect on the two sides of the body, namely, the normal erect posture. But the right and left bags are mirror pictures of each other. If the head incline to one side the resulting slip, microscopic though it be, of the two stones on their nerve-patches makes the stimulation unequal. And from that slip there results exactly the right unsymmetrical action of the muscles to give the unsymmetrical pose of limbs and neck required for stability. That is the mechanism dealing with limbs and trunk and neck. An additional one postures the head itself on the neck; a second pair of tiny gravity-bags, in which the stones hang rather than press. These, when any cause inclining the head has passed, bring the head back at once to the normal symmetry of the erect posture. And these same bags manage the posturing of the eyes. The eye contributes to our orientation in space; for instance, to perception of the vertical. And for this the eyeball, that is the retina, has to be postured normally. The pair of little gravity-bags in the skull, which act to restore the head posture, act also on the eyeball muscles. Whichever way the head turns, slopes, or is tilted, these adjust the eyeball's posture compensatingly, so that the retina still looks out upon its world from an approximately normal posture, retaining its old verticals and horizontals. As the head twists to the right the eyeball's visual axis untwists from the right. These reactions of head and eyes and body unconsciously take place when a bird wheels or slants in flight or a pilot stalls or banks his aeroplane. And all this works itself involuntarily as a pure mechanism, whose analysis we owe mainly to Prof. Magnus and Dr. de Kleijn, of Utrecht.

True, in such a glimpse of mechanism what we see mainly is how the machinery starts and what finally comes out of it; the intermediate elements of the process we know less of. Each insight into mechanism reveals more mechanism still to know. Thus, hardly was the animal's energy balance in its bearing upon food intake shown comfortably to conform with thermodynamics than came evidence of the so-called 'vitamines.' Unsuspected influence on nutrition by elements of diet taken in quantities so small as to make their mere calorie value quite negligible; thus, for

the growing rat, to quote Professor Harden, a quantity of vitamin A of the order of $\frac{1}{500}$ milligram a day. Again, as regards sex determination, the valued discovery of a visible distinction between the nuclear threads of male and female brings the further complexity that in such cases sex extends throughout the whole body to every dividing cell. Again, the association of hereditary unit-factors, such as body colour or shape of wing, to visible details in the segmenting nucleus seemed to simplify by epitomising. But further insight tends to trace the inherited unit character not to the chromosome itself, but to balance of action between the chromosome group. As with the atom in this heroic age of physicists, the elementary unit assumed simple proves, under further analysis, to be itself complex. Analysis opens a vista of further analysis required. Knowledge of muscle contraction has, from the work of Fletcher and Hopkins on to Hill, Hartree, Meyerhof, and others, advanced recently more than in many decades heretofore. The engineer would find it difficult to make a motive machine out of white of egg, some dissolved salts, and thin membrane. Yet this practically is what Nature has done in muscle, and obtained a machine of high mechanical efficiency. Perhaps human ingenuity can learn from it. One feature in the device is alternate development and removal of acidity. The cycle of contraction and relaxation lies traced to the production of lactic acid from glycogen and its neutralisation chiefly by alkaline proteins; and physically to an admirably direct transition from chemical to mechanical effect. What new steps of mechanism all this now opens! To arrive at one goal is to start for others.

But knowledge, while making for complexity, makes also for simplification. There seems promise of simplification as to the mechanism of reflex action. Reflex action with surprising nicety calls into play just the appropriate muscles, and adjusts them in time and in the suitable grading of their strength of pull. The moderating as well as the driving of muscles is involved. Also the muscles have to pass from the behest of one stimulus to that of another, even though the former stimulus still persist. For these gradings, coadjustments, restraints, and shifts various separate kinds of mechanism were assumed to exist in the nerve-centres, although of the nature of such mechanisms little could be said. Their processes were regarded as peculiar to the nerve-centres and different from anything that the simple fibres of nerve-trunks outside the centres can produce. We owe to Lucas and Adrian the demonstration that without any nerve-centre whatever an excised nerve-trunk with its muscle attached can be brought to yield, besides conduction of nerve impulses, the extinction or attenuation or augmentation of them. That is remarkable, because the impulse is not gradable by grading the strength of the stimulus. Any stimulus of strength sufficient to excite the nerve-fibre at all, excites in it an impulse which

is the fullest which the nerve-fibre can at the time give. The energy of the impulse comes not from the stimulus, but from the fibre itself. Lucas and Adrian have shown it gradable in another way. Though the nerve impulse is a quite brief affair—it lasts about $\frac{1}{1000}$ second at any one point of the nerve—it leaves behind it in the nerve-fibre a short phase during which the fibre cannot develop a second impulse. Then follows rapid but gradual recovery of the strength of impulse obtainable from the fibre. That recovery may swing past normal to super-normal before final return to the old resting state. Hence, by appropriately timing the arrival of a second impulse after a first, that second impulse may be extinguished or reduced or increased or transmitted without alteration. This property of grading impulses promises a complete key to reflex action if taken along with one other. The nervous system, including its centres, consists of nothing but chains of cells and fibres. In these chains the junctions of the links appear to be points across which a large impulse can pass, though a weak one will fail. At these points the grading of impulses by the interference process just outlined can lead, therefore, to narrowing or widening of their further distribution, much as in a railway system the traffic can be blocked or forwarded, condensed or scattered. Thus the distribution and quantity of the muscular effect can be regulated and shifted not only from one muscle to another, but in one and the same muscle can be graded by adding to or subtracting from the number of fibres activated within that muscle. As pointed out by Prof. Alexander Forbes, it may be, therefore, that the nerve impulse is the one and only reaction throughout the whole nervous system, central and peripheral, trains of impulses simply interfering, colliding and over-running as they travel along the inter-connected branches of the conductive network. In this may lie the secret of the co-ordination of reflexes. The nerve-centre seems nothing more than a meeting-place of nerve-fibres, its properties but those of impulses in combination. Fuller knowledge of the mechanism of the nervous impulse, many of whose physical properties are now known, a reaction open to study in the simplest units of the nervous system, thus leads to a view of nervous function throughout that system much simpler than formerly obtained.

Yet for some aspects of nervous mechanism the nerve impulse offers little or no clue. The fibres of nerve-trunks are perhaps of all nerve-structures those that are best known. They constitute, for instance, the motor nerves of muscle and the sensory nerves of the skin. When they are broken the muscle or skin is paralysed. They establish their ties with muscle and skin during embryonic life. These ties they then maintain practically unaltered throughout the individual's existence, and show no further growth. If severed, say, by a wound, they die

for their whole length between the point of severance and the muscle or skin they go to. And then at once the cut ends of the nerve-fibres start re-growing from the point of severance, although for years they have given no sign of growth. The fibre, so to say, tries to grow out to reach to its old far-distant muscle. There are difficulties in its way. A multitude of non-nervous repair cells growing in the wound spin scar tissue across the new fibre's path. Between these alien cells the new nerve-fibre threads a tortuous way, avoiding and never joining any of them. This obstruction it may take many days to traverse. Then it reaches a region where the sheath-cells of the old dead nerve-fibres lie altered beyond ordinary recognition. But the growing fibre recognises them. Tunnelling through endless chains of them, it arrives finally, after weeks or months, at the wasted muscle-fibres which seem to have been its goal, for it connects with them at once. It pierces their covering membranes and re-forms with their substance junctions of characteristic pattern resembling the original that had died weeks or months before. Then its growth ceases, abruptly, as it began, and the wasted muscle recovers and the lost function is restored.

Can we trace the causes of this beneficent yet so unaccountable reaction? How is it that severance can start the nerve re-growing? How does the nerve-fibre find its lost muscle microscopically miles away? What is the mechanism that drives and guides it? Is it a chemotaxis like that of the antherozooid in the botanical experiment drawn towards the focus of the dissolved malic acid? If so, there must be a marvellously arranged play of intricate sequences of chemically attractive and repellent substances dissolved suitably point to point along the tissue. It has recently been reported that the nerve-fibre growing from a nerve-cell in a nutrient field of graded electrical potential grows strictly by the axis of the gradient. Some argue for the existence of such potential gradients in the growing organism. Certainly nerve regeneration seems a return to the original phase of growth, and pieces of adult tissue removed from the body to artificial nutrient media in the laboratory take on vigorous growth. Professor Champy describes how epithelium that in the body is not growing when thus removed starts growing. If freed from all fibrous tissue its cells not only germinate, but, as they do so, lose their adult specialisation. In nerve regeneration the nerve-sheath cells, and to some extent the muscle-cells which have lost their nerve-fibre, lose likewise their specialised form, and regain it only after touch with the nerve-cell has been re-established. So similarly epithelium and its connective tissue cultivated outside the body together both grow and both retain their specialisation. All seems to argue that the mutual touch between the several cells of the body is decisive of much in their individual shaping and destiny. The severance of a nerve-fibre is an instance of the disloca-

tion of such a touch. It recalls well-known experiments on the segmenting egg. Destruction of one of the two halves produced by the first segmentation of the egg results in a whole embryo from the remaining half-egg. But if the two blastomeres, though ligated, be left side by side, each then produces a half-embryo. Each half-egg *can* yield a whole embryo, but is restrained by the presence of the twin cell to yielding but a half one. The nerve severance seems to break a mutual connection which restrained cell growth and maintained cell differentiation.

It may be said that the nerve-sheath cells degrade because absence of transmission of nerve impulses leaves their fibre functionless. But they do not degrade in the central nerve-piece, although impulses no longer pass along its afferent fibres. This mechanism of reconstruction seems strangely detached from any direct performance of function. The sprouting nerve-fibres of a motor nerve with impulses for muscular contraction can by misadventure take their way to denervated skin instead of muscle. They find the skin-cells whose nerve-fibres have been lost, and on these they bud out twigs, as true sensory fibres would do. Then, seemingly satisfied by so doing, they desist from further growth. The sense-cells, too, after this misunion, regain their normal features. But this joining of motor nerve-fibre with sense-cell is functionless, and must be so because the directions of functional conduction of the two are incompatible.

So, similarly, a regenerating skin-nerve led down to muscle makes its union with muscle instead of skin, though the union is a functional misfit, and cannot subserve function. Marvellous though nerve regeneration be, its mechanism seems blind. Its vehemence is just as great after amputation, when the parts lost can of course never be re-reached. Its blindness is sadly evident in the suffering caused by the useless nerve-sprouts entangled in the scar of a healing or healed limb-stump.

But there is a great difference between the growth of such regeneration and the growth impulse in pieces of tissue isolated from the body and grown in media outside. With pure cultures of these latter Professor Champy says the growth recalls in several features that of malignant tumours—multiplication of cells unaccompanied by formation of a specialised adult tissue. A piece of kidney cultivated outside the body de-differentiates, to use his term, into a growing mass unorganised for renal function. But with connective-tissue cells added even breast-cancer epithelium will in cultivation grow in glandular form. New ground is being broken in the experimental control of tissue growth. The report of the Imperial Cancer Research Fund mentions that in cultivation outside the body malignant cells present a difficulty that normal cells do not. To the malignant cells the nutrient soil has to be more frequently renewed, because they seem rapidly to

make the soil in which they grow poisonous to themselves, though not to normal cells. The following of all clues of difference between the mechanism of malignant growth and of normal is fraught with importance which may be practical as well as theoretical.

The regenerating nerve rebuilds to a plan that spells for future function. But throughout all its steps prior to the actual reaching the muscle or skin no actual performance of nerve-function can take place. What is constructed is functionally useless until the whole is complete. So, similarly, with much of the construction of the embryo in the womb for purposes of a different life after emergence from the womb; with the construction of the butterfly's wing within the chrysalis for future flight; of the lung for air-breathing after birth; of the reflex contraction in the fœtal child of the eyelids to protect the eye long before the two eyelids have been separated, let alone ere hurt or even light can reach it. The nervous system in its repair, as in its original growth, shows us a mechanism working through phases of non-functioning preparation in order to forestall and meet a future function. It is a mechanism against whose seeming prescience is to be set its fallibility and its limitations. The how of its working is at present chiefly traceable to us in the steps of its results rather than in comprehension of its intimate reactions; as to its mechanism, perhaps the point of chief import for us here is that those who are closest students of it still regard it as a mechanism. But if to know be to know the causes we must confess to want of knowledge of how its mechanism is contrived.

And if we knew the whole how of the production of the body from egg to adult, and if we admit that every item of its organic machinery runs on physical and chemical rules as completely as do inorganic systems, will the living animal present no other problematical aspect? The dog, our household friend—do we exhaust its aspects if in assessing its sum-total we omit its mind? A merely reflex pet would please little even the fondest of us. True, our acquaintance with other mind than our own can only be by inference. We may even hold that mind as an object of study does not come under the rubric of Natural Science at all. But this Association has its Section of Psychology, and my theme of to-night was partly chosen at the instance of a late member of it, Dr. Rivers, the loss of whom we all deplore. As a biologist he viewed mind as a biological factor. The keeping of mind and body apart for certain analytic purposes must not allow us to forget their being set together when we assess as a whole even a single animal life.

Taking as manifestations of mind those ordinarily received as such, mind does not seem to attach to life, however complex, where there is no nervous system, nor even where that system, though present, is quite scantily developed. Mind becomes more recognisable the more developed the nerve-system. Hence the difficulty of the twilit emer-

gence of mind from no mind, which is repeated even in the individual life history. In the nervous system there is what is termed localisation of function, relegation of different work to the system's different parts. This localisation shows mentality, in the usual acceptation of that term, not distributed broadcast throughout the nervous system, but restricted to certain portions of it—thus, among vertebrates to what is called the forebrain, and in higher vertebrates to the relatively newer parts of that forebrain. Its chief, perhaps its sole, seat is a comparatively modern nervous structure superposed on the non-mental and more ancient other nervous parts. The so-to-say mental portion of the system is placed so that its commerce with the body and the external world occurs only through the archaic non-mental rest of the system. Simple nerve impulses, their summations and interferences, seem the one uniform office of the nerve-system in its non-mental aspect. To pass from a nerve impulse to a psychical event, a sense-impression, percept, or emotion is, as it were, to step from one world to another and incommensurable one. We might expect, then, that at the places of transition from its non-mental to its mental regions the brain would exhibit some striking change of structure. But no; in the mental parts of the brain still nothing but the same old structural elements, set end to end, suggesting the one function of the transmission and collision of nerve impulses. The structural inter-connections are richer, but that is a merely quantitative change.

I do not want, and do not need, to stress our inability at present to deal with mental actions in terms of nervous actions, or *vice versa.* But facing the relation borne in upon us as existent between them, may we not gain some further appreciation of it by reminding ourselves even briefly of certain points of contact between the two? Familiar as such are, I will merely mention rather than dwell upon them.

One is the so-called expression of the emotions. The mental reaction of an emotion is accompanied by a nervous discharge which is more or less characteristic for each several type of emotion, so that the emotion can be read from its bodily expression. This nervous discharge is involuntary, and can affect organs, such as the heart, which the will cannot reach. Then there is the circumstance that the peculiar ways and tricks of the nervous machinery as revealed to us in the study of pure reflex reactions repeat themselves obviously in the working of the machinery to which mental actions are adjunct. The phenomenon of fatigue is common to both, and imposes similar disabilities on both. Nervous exhaustion and mental exhaustion mingle. Then, as offset against this disability, there exists in both the amenability to habit formation, mere repetition within limits rendering a reaction easier and readier. Then, and akin to this, is the oft-remarked trend in both for a reaction to leave behind itself a trace, an engram, a memory, the reflex engram, and the mental memory.

How should inertia and momentum affect non-material reactions? Quick though nervous reactions are, there is always easily observed delay between delivery of stimulus and appearance of the nervous end effect; and there is always the character that a reaction once set in motion does not cease very promptly. Just the same order of lag and overrun, of want of dead-beat character, is met in sense-reactions. The sensation outlives the light which evoked it and for longer the stronger the reaction. Just so the reflex after-discharge persists after the stimulus is withdrawn, and subsides more slowly the stronger the reaction. The times in both are of the same order. Again, a reflex act which contracts one muscle commonly relaxes another. Even so along with rise of sensation in one part of the visual field commonly occurs lapse of sensation in another. And the stoppage is in both by inhibition, that is to say, active. Then, again, two lights of opposite colour falling simultaneously and correspondingly on the two retinæ will, according to their balance, fuse to an intermediate tint or see-saw back and forth between the one tint and the other. Just similarly a muscle impelled by two reflexes, one tending to contract it, the other to relax it, will according to the balance of these respond steadily with an intensity, a compromise between the two, or see-saw rhythmically from extreme to extreme of the two opposite influences.

Reflex acts commonly predispose to their opposites. So, similarly, the visual impression of one colour predisposes to that of its opposite. Again, the *position* of the stimulated sensual point acts on the mind—hence the light seen or the pain felt is referred to some locus in the mind's space-system. Just similarly the reflex machinery directs, for instance, the limb it moves towards the particular spot stimulated. And such spots in the two processes, mental and non-mental, correspond.

Characteristic of the nervous machinery is its arrangement in what Hughlings Jackson called 'levels,' the higher levels standing to the lower not only as drivers but also as restrainers. Hence in disease underaction of one sort is accompanied by overaction of another. Thus in the arm affected by a cerebral stroke, besides loss of willed—that is higher level—power in the finger muscles, there is in other muscles involuntary overaction owing to escape of lower centres from control by the higher which have been destroyed. So, similarly, with the sensory effects. Of skin sensations some are painful and some not, for instance touch. The seat of the latter is of higher level, cortical; of the former lower, sub-cortical. When cerebral disease breaks the path between the higher and the underlying level a result is impairment of touch sensation but heightening of pain sensation in the affected part. The sensation of touch, as Dr. Head says, restrains that of pain.

Thus features of nervous working resemble over and over again mental. Is it mere metaphor when we speak of mental attitudes as

well as bodily? Is it mere analogy to liken the warped attitude of the mind in a psychoneurotic sufferer to the warped attitude of the body constrained by an internal potential pain? Again, some mental events seem spontaneous; in the nervous system some impulses seem generated automatically from within.

It may be said of all these similarities of time-relation and the rest between the ways of the nervous system and such simpler ways of mind as I here venture on, that they exist because the operations of the mental part of the nervous system communicate with the exterior only through the non-mental part as gateway—that there, then, the features of the nerve-machinery are impressed on the mind's working. But that suggestion forgets that the higher and more complex the mental process, the longer the time-lag, the more incident the fatigue, the more striking the memory character, and so on.

Yet all this similarity does but render more succinct the old enigma as to the nexus between nerve impulse and mental event. In the proof that the working of the animal mechanism conforms with the first law of thermodynamics can one say that psychical events are evaluated in the balance sheet drawn up? And, on the other hand, Mr. Barcroft and his fellow-observers in their recent physiological exploration of life on the Andes at 14,200 ft. noted that, as well as were their muscles, their arithmetic there was at a disadvantage. The low oxygen pressure militated against both. Indeed we all know that in any of us a few minutes without oxygen, or a few more with chloroform, and the psychical and the nervous events will lapse together. The nexus between the two sets of events is strict. But for comprehension of its nature we still require, it seems, comprehension of the unsolved mystery of the how of life itself. A shadowy bridge between them may lie perhaps in the reflection that for the observer himself the physical phenomena he observes are in the last resort psychical.

The practical man has to accept nervous function as a condition for mental function without breaking his heart over ignorance of their connection. The doctor, the lawyer, and we all, accept it. We know that with structural derangement or destruction of certain parts of the brain goes mental derangement or defect, while derangement or destruction of other parts of the nervous system is not so accompanied. Decade by decade the connection becomes more ascertained between certain mental performances and certain cerebral regions. Certain impairments of ideation as shown by forms of incomprehension of language or of familiar objects can help to diagnose for the surgeon as to what part of the brain a tumour is compressing; and the tumour gone the mental disabilities pass. So, similarly, those who, as Professor Elliott Smith and Sir Arthur Keith, recast the shape of the cerebrum from the cranial remains of prehistoric man can outline for us something

of his mentality from examination of the relative development of the several brain regions, using a true and scientific phrenology.

Could we look quite naively at the question of a seat for the mind within the body we might perhaps suppose it diffused there, not localised in any one particular part at all. That it is localised and that its localisation is in the nervous system—can we attach meaning to that fact? The nervous system is that bodily system whose special office from its earliest appearance onward throughout evolutionary history has been more and more to weld together the body's component parts into one consolidated mechanism reacting as a unity to the changeful world about it. It more than any other system has constructed out of a collection of organs an individual of unified act and experience. It represents the acme of accomplishment of the integration of the animal organism. That it is in this system that mind, as we know it, has had its beginning, and with the progressive development of the system has step for step developed, is surely significant. So is it that in this system the portion to which mind transcendently attaches is exactly that where are carried to their highest pitch the nerve-actions which manage the individual as a whole, especially in his reactions to the external world. There, in the brain, the integrating nervous centres are themselves further compounded, inter-connected, and re-combined for unitary functions. The cortex of the forebrain is the main seat of mind. That cortex with its twin halves corresponding to the two side-halves of the body is really a single organ knitting those halves together by a still further knitting together of the nervous system itself. The animal's great integrating system is there still further integrated. And this supreme integrator is the seat of all that is most clearly inferable as the animal's mind. As such it has spelt biological success to its possessors. From small beginnings it has become steadily a larger and larger feature of the nervous system, until in adult man the whole rest of the system is relatively dwarfed by it. Not without significance, perhaps, is that in man this organ, the brain cortex, bifid as it is, shows unmistakable asymmetry. Man is a tool-using animal, and tools demand asymmetrical, though attentive and therefore unified, acts. A nervous focus unifying such motor function will, in regard to a laterally bipartite organ, tend more to one half or the other. In man's cerebrum the preponderance of one-half—namely, the left—over the other may be a sign of unifying function.

It is to the psychologist that we must turn to learn in full the contribution made to the integration of the animal individual by mind. But each of us can, without being a professed psychologist, yet recognise one achievement in that direction which mental endowment has produced. Made up of myriads of microscopic cell-lives, individually born, feeding and breathing individually within the body, each one

of us nevertheless appears to himself a single entity, a unity experiencing and acting as one individual. In a way the more far-reaching and many-sided the reactions of which a mind is capable the more need, as well as the more scope, for their consolidation to one. True, each one of us is in some sense not one self, but a multiple system of selves. Yet how closely those selves are united and integrated to one personality. Even in those extremes of so-called double personality one of their mystifying features is that the individual seems to himself at any one time wholly either this personality or that, never the two commingled. The view that regards hysteria as a mental dissociation illustrates the integrative trend of the total healthy mind. Circumstances can stress in the individual some perhaps lower instinctive tendency that conflicts with what may be termed his normal personality. This latter, to master the conflicting trend, can judge it in relation to his main self's general ethical ideals and duties to self and the community. Thus intellectualising it, he can destroy it or consciously subordinate it to some aim in harmony with the rest of his personality. By so doing there is gain in power of will and in personal coherence of the individual. But if the morbid situation be too strong or the mental self too weak, instead of thus assimilating the contentious element the mind may shun and, so to say, endeavour to ignore it. That way lies danger. The discordant factor escaped from the sway of the conscious mind produces stress and strain of the conscious self; hence, to use customary terminology, dissociation of the self sets in, bringing in its train those disabilities, mental or nervous or both, which characterise the sufferer from hysteria. The normal action of the mind is to make up from its components one unified personality. When we remember the manifold complexity of composition of the human individual, can we observe a greater instance of solidarity of working of an organism than that presented by the human individual intent and concentrated, as the phrase goes, upon some higher act of strenuous will? Physiologically the supreme development of the brain, psychologically the mental powers attaching thereto, seem to represent from the biological standpoint the very culmination of the integration of the animal organism.

The mental attributes of the nervous system would be, then, the coping-stone of the construction of the individual. Surveyed in their broad biological aspect, we see them carrying integration even further still. They do not stop at the individual; they proceed beyond the individual; they integrate from individuals communities. When we review, as far as we can judge it, the distribution of mind within the range of animal forms, we meet two peaks of its development—one in insect life, the other in the vertebrate, with its acme finally in man. True, in the insect the type of mind is not rational but instinctive, whereas

at the height of its vertebrate development reason is there as well as instinct. Yet in both one outcome seems to be the welding of individuals into societies on a scale of organisation otherwise unattained. The greatest social animal is man; the powers that make him so are mental—language, tradition, instinct for the preservation of the community, as well as for the preservation of the individual; reason actuated by emotion and sentiment and controlling and welding egoistic and altruistic instincts into one broadly harmonious, instinctive-rational behaviour. Just as the organisation of the cell-colony into an animal individual receives its highest contribution from the nervous system, so the further combining of animal individuals into a multi-individual organism, a social community, merging the interests of the individual in the interests of the group, is due to the nervous system's crowning attributes, the mental. That this integration is still in process, still developing, is obvious from the whole course of human pre-history and history. The biological study of it is essentially psychological; it is the scope and ambit of social psychology. Not the least important form of social psychology is that relatively new one, of which the President of the Psychology Section at this meeting is a foremost authority and exponent, namely, that dealing with the stresses and demands that organised industry makes upon the individual as a unit in the community of our day, and with the readjustments it asks from that community.

To resume, then, we may, I think, conclude that in some of its aspects animal life presents to us mechanism the how of which, despite many gaps in our knowledge, is fairly explicable. Of not a few of the processes of the living body, such as muscular contraction, the circulation of the blood, the respiratory intake and output by the lungs, the nervous impulse and its journeyings, we may fairly feel from what we know of them already that further application of physics and chemistry will furnish a competent key. We may suppose that in the same sense as we can claim to-day that the principles of working of a gas-engine or an electro-motor are comprehensible to us, so will the bodily working in such mechanisms be understood by us, and indeed are largely so already. It may well be possible to understand the principle of a mechanism which we have not the means or skill ourselves to construct. We cannot construct the atoms of a gas-engine. But, turning to other aspects of animal mechanism, such as the shaping of the animal body, the conspiring of its structural units to compass later functional ends, the predetermination of specific growth from egg to adult, the predetermined natural term of existence, these, and their intimate mechanism, we are, it seems to me, despite many brilliant inquiries and inquirers, still at a loss to understand. The steps of the results are known, but the springs

of action still lie hidden. Then, again, the how of the mind's connection with its bodily place seems still utterly an enigma. Similarity or identity in time-relations and in certain other ways between mental and nervous processes does not enlighten us as to the actual nature of the connection existent between the two. Advance in biological science does but serve to stress further the strictness of the nexus between the two.

Great differences of difficulty therefore confront our understanding of different aspects of animal life. Yet the living creature is fundamentally a unity. In trying to make the how of an animal existence intelligible to our imperfect knowledge we have for purposes of study to separate its whole into part-aspects and part-mechanisms, but that separation is artificial. It is as a whole, a single entity, that the animal, or for that matter the plant, has finally and essentially to be envisaged. We cannot really understand its one part without its other. Can we suppose a unified entity which is part mechanism and part not? One privilege open to the human intellect is to attempt to comprehend, not leaving out of account any of its properties, the how of the living creature as a whole. The problem is ambitious, but its importance and its reward are all the greater if we seize and we attempt the full width of its scope. In the biological synthesis of the individual it regards mind. It includes examination of man himself as acting under a biological trend and process which is combining individuals into a multi-individual organisation, a social organism surely new in the history of the planet. For this biological trend and process is constructing a social organism whose cohesion depends mainly on a property developed so specifically in man as to be, broadly speaking, his alone—namely, a mind actuated by instincts but instrumented with reason. Man, often Nature's rebel, as Sir Ray Lankester has luminously said, can, viewing this great supra-individual process, shape even as individual his course conformably with it, feeling that in this instance to rebel would be to sink lower rather than to continue his own evolution upward.

SECTIONAL ADDRESSES.

THE THEORY OF NUMBERS.

ADDRESS TO SECTION A (MATHEMATICS AND PHYSICS) BY

PROFESSOR G. H. HARDY, M.A., F.R.S.,

PRESIDENT OF THE SECTION.

I FIND myself to-day in the same embarrassing position in which a predecessor of mine at Oxford found himself at Bradford in 1875, the President of a Section which is probably the largest and most heterogeneous in the Association, and which is absorbed by a multitude of divergent professional interests, none of which agree with his or mine.

There are two courses possible in such circumstances. One is to take refuge, as Professor Henry Smith, with visible reluctance, did then, in a series of general propositions to which mathematicians, physicists, and astronomers may all be expected to return a polite assent. The importance of science and scientific method, the need for better organisation of scientific education and research, are all topics on which I could no doubt say something without undue strain either on my own honesty or on your credulity. That there is no finer education and discipline than natural science; that it is, as Dr. Campbell has said, 'the noblest of the arts'; that the crowning achievements of science lie in those directions with which this Section is professionally concerned: all this I could say with complete sincerity, and, if I were the head of a deputation approaching a Government Department, I suppose that I would not shirk even so unprofitable a task.

It is unfortunate that these essential and edifying truths, important as it is that they should be repeated as loudly as possible from time to time, are, to the man whose interest in life lies in scientific work and not in propaganda, unexciting, and in fact quite intolerably dull. I could, if I chose, say all these things, but, even if I wanted to, I should hardly increase your respect for mathematics and mathematicians by repeating to you what you have said yourselves, or read in the newspapers, a hundred times already. I shall say them all some day; the time will come when we shall none of us have anything more interesting to say. We need not anticipate our inevitable end.

I propose therefore to adopt the alternative course suggested by my predecessor, and to try to say something to you about something about which I have something to say. There is only one subject about which I have anything to say, and that is pure mathematics. It happens, by a fortunate accident, that the particular subject which I love the most, and which presents most of the problems which occupy my own researches, is by no means overwhelmingly recondite or obscure, and indeed is sharply distinguished from almost every other branch of pure mathematics, in that it makes a direct, popular, and almost irresistible appeal to the heart of the ordinary man.

There is, however, one preliminary remark which I cannot resist the temptation of making. The present is a particularly happy moment for a pure mathematician, since it has been marked by one of the greatest recorded triumphs of pure mathematics. This triumph is the work, as it happens, of a man who would probably not describe himself as a mathematician, but who has done more than any mathematician to vindicate the dignity of mathematics, and to put that obscure and perplexing construction, commonly described as 'physical reality,' in its proper place.

There is probably less difference between the methods of a physicist and a mathematician than is generally supposed. The most striking among them seems to me to be this, that the mathematician is in much more direct contact with reality. This may perhaps seem to you a paradox, since it is the physicist who deals with the subject-matter to which the epithet 'real' is commonly applied. But a very little reflexion will show that the 'reality' of the physicist, whatever it may be (and it is extraordinarily difficult to say), has few or none of the attributes which common-sense instinctively marks as real. A chair may be a collection of whirling atoms, or an idea in the mind of God. It is not my business to suggest that one account of it is obviously more plausible than the other. Whatever the merits of either of them may be, neither draws its inspiration from the suggestions of common-sense.

Neither the philosophers nor the physicists themselves have ever put forward any very convincing account of what physical reality is, or of how the physicist passes, from the confused mass of fact or sensation from which he starts, to the construction of the objects which he classifies as real. We cannot be said, therefore, to know what the subject-matter of physics is; but this need not prevent us from understanding the task which a physicist is trying to perform. That, clearly, is to correlate the incoherent body of facts confronting him with some definite and orderly scheme of abstract relations, the kind of scheme, in short, which he can only borrow from mathematics.

A mathematician, on the other hand, fortunately for him, is not concerned with this physical reality at all. It is impossible to prove, by mathematical reasoning, any proposition whatsoever concerning the physical world, and only a mathematical crank would be likely now to imagine it his function to do so. There is plainly one way only of ascertaining the facts of experience, and that is by observation. It is not the business of a mathematician to suggest one view of the universe or another, but merely to supply the physicists with a collection of abstract schemes, which it is for them to select from, and to adopt or discard at their pleasure.

The most obvious example is to be found in the science of geometry. Mathematicians have constructed a very large number of different systems of geometry, Euclidean or non-Euclidean, of one, two, three, or any number of dimensions. All these systems are of complete and equal validity. They embody the results of mathematicians' observations of *their* reality, a reality far more intense and far more rigid than the dubious and elusive reality of physics. The old-fashioned geometry of Euclid, the entertaining seven-point geometry of Veblen, the space-

times of Minkowski and Einstein, are all absolutely and equally real. When a mathematician has constructed, or, to be more accurate, when he has observed them, his professional interest in the matter ends. It may be the seven-point geometry that fits the facts the best, for anything that mathematicians have to say. There may be three dimensions in this room and five next door. As a professional mathematician, I have no idea; I can only ask the Secretary, or some other competent physicist, to instruct me in the facts.

The function of a mathematician, then, is simply to observe the facts about his own hard and intricate system of reality, that astonishingly beautiful complex of logical relations which forms the subject-matter of his science, as if he were an explorer looking at a distant range of mountains, and to record the results of his observations in a series of maps, each of which is a branch of pure mathematics. Many of these maps have been completed, while in others, and these, naturally, the most interesting, there are vast uncharted regions. Some, it seems, have some relevance to the structure of the physical world, while others have no such tangible application. Among them there is perhaps none quite so fascinating, with quite the same astonishing contrasts of sharp outline and mysterious shade, as that which constitutes the theory of numbers.

The number system of arithmetic is, as we know too well, not without its applications to the sensible world. The currency systems of Europe, for example, conform to it approximately; west of the Vistula, two and two make something approaching four. The practical applications of arithmetic, however, are tedious beyond words. One must probe a little deeper into the subject if one wishes to interest the ordinary man, whose taste in such matters is astonishingly correct, and who turns with joy from the routine of common life to anything strange and odd, like the fourth dimension, or imaginary time, or the theory of the representation of integers by sums of squares or cubes.

It is impossible for me to give you, in the time at my command, any general account of the problems of the theory of numbers, or of the progress that has been made towards their solution even during the last twenty years. I must adopt a much simpler method. I will merely state to you, with a few words of comment, three or four isolated questions, selected in a quite haphazard way. They are seemingly simple questions, and it is not necessary to be anything of a mathematician to understand them; and I have chosen them for no better reason than that I happen to be interested in them myself. There is no one of them to which I know the answer, nor, so far as I know, does any mathematician in the world; and there is no one of them, with one exception which I have included deliberately, the answer to which any one of us would not make almost any sacrifice to know.

1. *When is a number the sum of two cubes, and what is the number of its representations?* This is my first question, and first of all I will elucidate it by some examples. The numbers $2=1^3+1^3$ and $9=2^3+1^3$ are sums of two cubes, while 3 and 4 are not: it is exceptional for a number to be of this particular form. The number of cubes up to 1000000 is 100, and the number of numbers, up to this limit and of the form required, cannot exceed 10000, one-hundredth of

the whole. The density of the distribution of such numbers tends to zero as the number tends to infinity. Is there, I am asking, any simple criterion by which such numbers can be distinguished?

Again, 2 and 9 are sums of two cubes, and can be expressed in this form in one way only. There are numbers so expressible in a variety of different ways. The least such number is 1729, which is 12^3+1^3 and also 10^3+9^3. It is more difficult to find a number with *three* representations; the least such number is

$$175959000 = 560^3 + 70^3 = 552^3 + 198^3 = 525^3 + 315^3.$$

One number at any rate is known with *four* representations, viz.

$$19 \times 363510^3$$

(a number of 18 digits), but I am not prepared to assert that it is the least. No number has been calculated, so far as I know, with more than four, but theory, running ahead of computation, shows that numbers exist with five representations, or six, or any number.

A distinguished physicist has argued that the possible number of isotopes of an element is probably limited because, among the ninety or so elements at present under observation, there is none which has more isotopes than six. I dare not criticise a physicist in his own field; but the figures I have quoted may suggest to you that an arithmetical generalisation, based on a corresponding volume of evidence, would be more than a little rash.

There are similar questions, of course, for squares, but the answers to these were found long ago by Euler and by Gauss, and belong to the classical mathematics. Suppose, for simplicity of statement, that the number in question is *prime*. Then, if it is of the form $4m+1$, it is a sum of squares, and in one way only, while if it is of the form $4m+3$ it is not so expressible; and this simple rule may readily be generalised so as to apply to numbers of any form. But there is no similar solution for our actual problem, nor, I need hardly say, for the analogous problems for fourth, fifth, or higher powers. The smallest number known to be expressible in two ways by two biquadrates is

$$635318657 = 158^4 + 59^4 = 134^4 + 133^4;$$

and I do not believe that any number is known expressible in three. Nor, to my knowledge, has the bare existence of such a number yet been proved. When we come to fifth powers, nothing is known at all. The field for future research is unlimited and practically untrodden.

2. I pass to another question, again about cubes, but of a somewhat different kind. *Is every large number* (every number, that is to say, from a definite point onwards) *the sum of five cubes?* This is another exceptionally difficult problem. It is known that every number, without exception, is the sum of nine cubes; two numbers, 23 (which is $2.2^3+7.1^3$) and 239, actually require so many. It seems that there are just fifteen numbers, the largest being 454, which need eight, and 121 numbers, the largest being 8042, which need seven; and the evidence suggests forcibly that the six-cube numbers also ultimately disappear. In a lecture which I delivered on this subject at Oxford I stated, on the authority of Dr. Ruckle, that there were two numbers, in the

immediate neighbourhood of 1000000, which could not be resolved into fewer cubes than six; but Dr. A. E. Western has refuted this assertion by resolving each of them into five, and is of opinion, I believe, that the six-cube numbers have disappeared entirely considerably before this point. It is conceivable that the five-cube numbers also disappear, but this, if it be so, is in depths where computation is helpless. The four-cube numbers must certainly persist for ever, for it is impossible that a number $9n+4$ or $9n+5$ should be the sum of three.

I need hardly add that there is a similar problem for every higher power. For fourth powers the critical number is 16. There is no case, except the simple case of squares, in which the solution is in any sense complete. About the squares there is no mystery; every number is the sum of four, and there are infinitely many which cannot be expressed by fewer.

3. I will next raise the question *whether the number* $2^{137}-1$ *is prime*. I said that I would include one question which did not interest me particularly, and I should like to explain to you the kind of reasons which damp down my interest in this one. I do not know the answer, and I do not care greatly what it is.

The problem belongs to the theory of the so-called 'perfect' numbers, which has exercised mathematicians since the times of the Greeks. A number is perfect if, like 6 or 28, it is the sum of all its divisors, unity included. Euclid proved that the number

$$2^m(2^{m+1}-1)$$

is perfect if the second factor is prime; and Euler, 2,000 years later, that all *even* perfect numbers are of Euclid's form. It is still unknown whether a perfect number can be odd.

It would obviously be most interesting to know generally in what circumstances a number 2^n-1 is prime. It is plain that this can only be so if n itself is prime, as otherwise the number has obvious factors; and the 137 of my question happens to be the least value of n for which the answer is still in doubt. You may perhaps be surprised that a question apparently so fascinating should fail to arouse me more.

It was asserted by Mersenne in 1644 that the only values of n, up to 257, for which 2^n-1 is prime are

$$2, 3, 5, 7, 13, 17, 19, 31, 67, 127, 257;$$

and an enormous amount of labour has been expended on attempts to verify this assertion. There are no simple general tests by which the primality of a number chosen at random can be determined, and the amount of computation required in any particular case may be quite appalling. It has, however, been imagined that Mersenne perhaps knew something which later mathematicians have failed to rediscover. The idea is a little fantastic, but there is no doubt that, so long as the possibility remained, arithmeticians were justified in their determination to ascertain the facts at all costs. 'The riddle as to how Mersenne's numbers were discovered remains unsolved,' wrote Mr. Rouse Ball in 1891. Mersenne, he observes, was a good mathematician, but not an Euler or a Gauss, and he inclines to attribute the discovery to the exceptional genius of Fermat, the only mathematician

of the age whom anyone could suspect of being hundreds of years ahead of his time.

These speculations appear extremely fanciful, for the bubble has at last been pricked. It seems now that Mersenne's assertion, so far from hiding unplumbed depths of mathematical profundity, was a conjecture based on inadequate empirical evidence, and a rather unhappy one at that. It is now known that there are at least four numbers about which Mersenne is definitely wrong; he should have included at any rate 61, 89, and 107, and he should have left out 67. The mistake as regards 61 and 67 was discovered as long ago as 1886, but could be explained with some plausibility, so long as it stood alone, as a merely clerical error. But when Mr. R. E. Powers, in 1911 and 1914, proved that Mersenne was also wrong about 89 and 107, this line of defence collapsed, and it ceased to be possible to take Mersenne's assertion seriously.

The facts may be summed up as follows. Mersenne makes fifty-five assertions, for the fifty-five primes from 2 to 257. Of these assertions forty are true, four false, and eleven still doubtful. Not a bad result, you may think; but there is more to be said. Of the forty correct assertions many, half at least, are trivial, either because the numbers in question are comparatively small, or because they possess quite small and easily detected divisors. The test cases are those in which Mersenne asserts the numbers in question to be prime; there are only four of these cases which are difficult and in which the truth is known; and in these Mersenne is wrong in every case but one.

It seems to me, then, that we must regard Mersenne's assertion as exploded; and for my part it interests me no longer. If he is wrong about 89 and 107, I do not care greatly whether he is wrong about 137 as well or not, and I should regard the computations necessary to decide as very largely wasted. There are so many much more profitable calculations which a computer could undertake.

I hope that you will not infer that I regard the problem of perfect numbers as uninteresting in itself; that would be very far from the truth. There are at least two intensely interesting problems. The first is the old problem, which so many mathematicians have failed to solve, whether a perfect number can be odd. The second is whether the number of perfect numbers is infinite or not. If we assume that all perfect numbers are infinite, we can state this problem in a still more arresting form. *Are there infinitely many primes of the form* $2^n - 1$? I find it hard to imagine a problem more fascinating or more terribly difficult than that. It is plain, though, that this is a question which computation can never decide, and it is very unlikely that it can ever give us any data of serious value. And the problem itself really belongs to a different chapter of the theory, to which I should like next to direct your attention.

4. *Are there infinitely many primes of the form* n^2+1? Let me first remind you of some well-known facts in regard to the distribution of primes.

There are infinitely many primes; their density decreases as the numbers increase, and tends to zero when the numbers tend to infinity.

More accurately, the number of primes less than x is, to a first approximation,

$$\frac{x}{\log x}.$$

The chance that a large number n, selected at random, should be prime is, we may say, about $\frac{1}{\log n}$. Still more precisely, the 'logarithm-integral'

$$\operatorname{Li} x = \int_2^x \frac{dt}{\log t}$$

gives a very good approximation to the number of primes. This number differs from Li x by a function of x which oscillates continually, as Mr. Littlewood, in defiance of all empirical evidence to the contrary, has shown, between positive and negative values, and is sometimes large, of the order of magnitude $\sqrt{x}$ or thereabouts, but always small in comparison with the logarithm-integral itself.

Except for one lacuna, which I must pass over in silence now, this problem of the general distribution of primes, the first and central problem of the theory, is in all essentials solved. But a variety of most exciting problems remain as to the distribution of primes among numbers of special forms. The first and simplest of these is that of the arithmetical progressions: *How are the primes distributed among all possible arithmetical progressions* $an+b$? We may leave out of account the case in which a and b have a common factor; this case is trivial, since $an+b$ is then obviously not prime.

The first step towards a solution was made by Dirichlet, who proved for the first time, in 1837, that any such arithmetical progression contains an infinity of primes. It has since been shown that the primes are, to a first approximation at any rate, distributed evenly among all the arithmetical progressions. When we pursue the analysis further differences appear; there are on the average, for example, more primes $4n+3$ than primes $4n+1$, though it is not true, as the evidence of statistics has led some mathematicians to conclude too hastily, that there is always an excess to whatever point the enumeration is carried.

The problem of the arithmetical progressions, then, may also be regarded as solved; and the same is true of the problem of the primes of a given quadratic form, say $am^2+2bmn+cn^2$, homogeneous in the two variables m and n. To take, for instance, the simplest and most striking case, there is the natural and obvious number of primes m^2+n^2. A prime is of this form, as I have mentioned already, if and only if it is of the form $4k+1$. The quadratic problem reduces here to a particular case of the problem of the arithmetical progression.

When we pass to cubic forms, or forms of higher degree, we come to the region of the unknown. This, however, is not the field of inquiry which I wish now to commend to your attention. The quadratic forms of which I have spoken are forms in two independent variables m and n; the form n^2+1 of my question is a non-homogeneous form in a single variable n, the simplest case of the general form $an^2+2bn+c$. It is clear that one may ask the same question for forms of any degree:

Are there, for example, infinitely many primes n^3+2 or n^4+1? I do not choose n^3+1, naturally, because of the obvious factor $n+1$.

This problem is one in which computation can still play an important part. You will remember that I stated the same problem for perfect numbers. There a computer is helpless. For the numbers 2^n-1, which dominate the theory, increase with quite unmanageable rapidity, and the data collected by the computers appear, so far as one can judge, to be almost devoid of value. Here the data are ample, and, though the question is still unanswered, there is really strong statistical evidence for supposing a particular answer to be true. It seems that the answer is affirmative, and that there is a definite approximate formula for the number of primes in question. This formula is

$$\frac{1}{2}\mathrm{Li}\sqrt{x}\times\left(1+\frac{1}{3}\right)\left(1-\frac{1}{5}\right)\left(1+\frac{1}{7}\right)\left(1+\frac{1}{11}\right)\ldots\ldots,$$

where the product extends over all primes p, and the positive sign is chosen when p is of the form $4n+3$. Dr. A. E. Western has submitted this formula to a most exhaustive numerical check. It so happens that Colonel Cunningham some years ago computed a table of primes n^2+1 up to the value 15,000 of n, a limit altogether beyond the range of the standard factor tables, and Cunningham's table has made practicable an unusually comprehensive test. The actual number of primes is 1199, while the number predicted is 1219. The error, less than 1 in 50, is much less than one could reasonably expect. The formula stands its test triumphantly, but I should be deluding you if I pretended to see any immediate prospect of an accurate proof.

5. The last problem I shall state to you is this: *Are there infinitely many prime-pairs p, $p+2$?* One may put the problem more generally: *Does any group of primes, with assigned and possible differences, recur indefinitely, and what is the law of its recurrence?*

I must first explain what I mean by a 'possible' group of primes. It is possible that p and $p+2$ should both be prime, like 3, 5, or 101, 103. It is not possible (unless p is 3) that p, $p+2$ and $p+4$ should *all* be prime, for one of them must be a multiple of 3: but p, $p+2$, $p+6$ or p, $p+4$, $p+6$ are possible triplets of primes. Similarly

$$p,\ p+2,\ p+6,\ p+8,\ p+12$$

can all be prime, so far as any elementary test of divisibility shows, and in fact 5, 7, 11, 13 and 17 satisfy the conditions. It is easy to define precisely what we understand by a 'possible' group. We mean a group whose differences, like 0, 2, 6, have at least one missing residue to every possible modulus. The 'impossible' group 0, 2, 4 does not satisfy the condition, for the remainders after division by 3 are 0, 2, 1, a complete set of residues to modulus 3. There is no difficulty in specifying possible groups of any length we please.

We define in this manner, then, a 'possible' group of primes, and we put the questions: Do all possible groups of primes actually occur, do they recur indefinitely often, and how often on the average do they recur? And here again it would seem that the answers are affirmative, that all possible groups occur, and continue to occur for ever, and with a frequency whose law can be assigned. The order of magnitude

of the number of prime-pairs, p, $p+2$, or p, $p+4$, or p, $p+6$, both of whose members are less than a large number x, is, it appears,

$$\frac{x}{(\log x)^2}.$$

The order of magnitude of the corresponding number of triplets, of any possible type, is

$$\frac{x}{(\log x)^3},$$

and so on generally. Further, we can assign the relative frequencies of pairs or triplets of different types; there are, for example, about twice as many pairs whose difference is 6 as pairs whose difference is 2. All these results have been tested by actual enumeration from the factor tables of the first million numbers; and a physicist would probably regard them as proved, though we of course know very well that they are not.

There is a great deal of mathematics the purport of which is quite impossible for any amateur to grasp, and which, however beautiful and important it may be, must always remain the possession of a narrow circle of experts. It is the peculiarity of the theory of numbers that much of it could be published broadcast, and would win new readers for the *Daily Mail*. The positive integers do not lie, like the logical foundations of mathematics, in the hardly visible distance, nor in the uncomfortably tangled foreground, like the immediate data of the physical world, but at a decent middle distance, where the outlines are clear and yet some element of mystery remains. There is no one so blind that he does not see them, and no one so sharp-sighted that his vision does not fail; they stand there a continual and inevitable challenge to the curiosity of every healthy mind. I have merely directed your attention for a moment to a few of the less immediately conspicuous features of the landscape, in the hope that I might sharpen your curiosity a little, and that some of you perhaps might feel tempted to walk a little nearer and take a rather closer view.

THE ORGANISATION OF RESEARCH.

ADDRESS (PART I) TO SECTION B (CHEMISTRY) BY

PRINCIPAL J. C. IRVINE, C.B.E., D.Sc., LL.D., F.R.S.,

PRESIDENT OF THE SECTION.

I AM deeply sensible of the honour done to me in electing me to this chair, and am well aware of my own unworthiness to occupy the position. Nevertheless, I feel that there is something appropriate in the choice which brings once more into close relationship the University of St. Andrews and the British Association. You will forgive me if, for the moment, my thoughts are focussed not so much on the subject assigned to our Section as on the origin and nature of this annual gathering of scientists.

The British Association was the product of an age rather than the inspiration of any one man, yet of those who first gave practical effect to the movement which has spread scientific learning and has bound its devotees in a goodly fellowship there was no more eager spirit than Sir David Brewster. It is not an exaggerated claim that it was he who founded the British Association. One may trace his enlightened action to a desire to combat the apathy and distrust shown by the Government of his day towards scientific work and even scientific men. Only in the historical sense can I claim any relationship with Brewster. It is my privilege to occupy the Principalship he once held, and I cannot escape from the thought that the daily tasks now mine were once his.

It is thus inevitable that to-day a name often in my mind should spring once more into recollection, especially as my distinguished predecessor was present at the first Hull meeting in 1853, when he contributed two papers to Section A. Chemists should be among the first to pay grateful tribute to Brewster's efforts on behalf of science, and I propose, therefore, to include in my address a review of the position scientific chemistry has won since his day in public and official estimation. Moreover, at the express suggestion of some of our members whose opinions cannot be disregarded, I am induced to add the consideration of the new responsibilities chemists have incurred now that so many of Brewster's hopes have been realised. These were recently submitted by me to another audience and, through the medium of an article in 'Nature,' are possibly known to you already, but I agree with my advisers that their importance warrants further elaboration and wider discussion.

It would be idle to recall the lowly position of chemistry as an educative force in this country, or to reconstruct the difficulties with which the scientific chemist was confronted during the first thirty years

of the nineteenth century. Present difficulties are serious enough, and press for all our attention, without dwelling unduly on troubles of the past. But we must at least remember that in the early days of the British Association 'schools' of chemistry were in their infancy, and that systematic instruction in the science was difficult to obtain. Another point of fundamental importance which has to be borne in mind is that the masters of the subject were then for the most part solitary workers.

It is difficult for us, looking back through the years, to realise what it must have meant to search for truth under conditions which were discouraging, if not actually hostile. Yet, although his labours were often thankless and unrewarded, the chemist of the time was probably a riper philosopher and a finer enthusiast than his successor of to-day. He pursued his inquiries amidst fewer distractions, and in many ways his lot must have been happy, save when tormented by the thought that a subject so potent as chemistry in developing the intellectual and material welfare of the community should remain neglected to an extent which to us seems incredible.

Public sympathy was lacking, Government support was negligible or grudgingly bestowed, and there was little or no co-operation between scientific chemistry and industry. As an unaided enthusiast the chemist was left to pursue his way without the stimulus, now happily ours, which comes from the feeling that work is supported by educated and enlightened appreciation.

Let me quote from one of Faraday's letters now in my possession and, so far as I can trace, unpublished. Writing to a friend immediately before the foundation of the British Association, he relates that a manufacturer had adopted a process developed in the course of an investigation carried out in the Royal Institution. The letter continues: 'He' (the manufacturer) 'writes me word that, having repeated our experiments, he finds the product very good, and as our information was given openly to the world he, as a matter of compliment, has presented me with some pairs of razors to give away.' If ever there was a compliment which could be described as empty, surely this was one; yet the letter gives the impression that Faraday himself was quite content with his reward.

It is perhaps unfair to quote Faraday as a type, for few men are blessed with his transparent simplicity of character, but there is obviously a great gulf fixed between the present day and a time when a debt of honour could be cancelled in such a manner. A little reflection will show that the British Association has played a useful part in discrediting the idea that because so much scientific discovery is given 'openly to the world,' those who profit by such discoveries should be absolved from their reasonable obligations. Even where scientific workers do not expect or desire personal reward, the institutions which provide them with their facilities are often sorely in need. The recognition, not yet complete, but more adequate than once was the case, that the labourer is worthy of his hire represents only one minor change which the years have brought.

An even greater contrast, embodying more important principles, is

found in the changed attitude of the State towards scientific education and discovery. Remember Brewster's fond hope that, by means of our Association, the whole status of science would be raised, and that a greater measure of support and encouragement would be received from the Government. How eagerly the venerable physicist must have listened to the Presidential Address delivered at the twenty-third meeting of the Association assembled in Hull for the first time! It dealt with many problems familiar to him. No doubt he followed with keen interest the account of the observations on Nebulæ made with Lord Rosse's telescope, and appreciated the references to the work of Joule and Thomson. The address was a masterly synopsis of scientific progress, but from time to time a new note steals in. There is a significant reference to a consultation with the Chancellor of the Exchequer, another to a conversation with Mr. Gladstone, and a third to a working arrangement concluded with the Admiralty. These would fall sweetly on Brewster's ear, and he would cordially approve of the report of our Parliamentary Committee which had established sympathetic contact with the House of Commons. He could not fail to be impressed with the changes a few years had brought.

Let us bridge the further gap of sixty-nine years which separates us from that day. The contrast is amazing, and once more we can trace the steady, persistent influence of the British Association in bringing about what is practically a revolution in public and official opinion. We have learned many lessons. The change has come suddenly, but it was not spontaneous. Many years had to be spent in disseminating the idea that research is a vital necessity, and toward this end Presidents of our Association have not hesitated, year after year, to add the weight of their influence and eloquence. It was courageous of them to do so. I would refer you particularly to the forcible appeals made by Sir James Dewar at Belfast and Sir Norman Lockyer at Southport, when the plea for more research was laid before the Association, and thus found its way by the most direct channel to the Press and to the public. No doubt many other factors have played a part in creating a research atmosphere in this country, but the steady pressure exerted by the British Association is not the least important of these influences.

The principles of science are to-day widely spread; systematic scientific training has found an honourable place in the schools and in the colleges; above all, there is the realisation that much of human progress is based on scientific inquiry, and at last this is fostered, and, in part, financed as a definite unit of national educational policy. Public funds are devoted to provide facilities for those who are competent to pursue scientific investigations, and in this way the State, acting through the Department of Scientific and Industrial Research, has assumed the double responsibility of providing for the advancement of knowledge and for the application of scientific methods to industry. Scientists have been given the opportunities they desired, and it remains for us to justify all that has been done. We have this morning glanced briefly at the painful toil and long years of preparation; now it falls to us to sow the first crop and reap the first harvest.

Thanks to the wisdom and foresight of others, it has been possible to frame the Government policy in the light of the experience gained with pre-existing research organisations. The pioneer scheme of the kind is that administered by the Commissioners of the 1851 Exhibition, who since 1890 have awarded research scholarships to selected graduates. When in 1901 Mr. Carnegie's benefaction was applied to the Scottish Universities the trustees wisely determined to devote part of the revenues to the provision of research awards which take the form of Scholarships, Fellowships, and Research Lectureships. These have proved an immense boon to Scottish graduates, and the success of the venture is sufficiently testified by the fact that the Government Research scheme was largely modelled on that of the Carnegie Trust.

In each of these organisations chemistry bulks largely, and the future of our subject is intimately connected with their success or failure. The issue lies largely in our hands. We must not forget that we are only at the beginning of a great movement, and that fresh duties now devolve upon us. It was my privilege for some years to direct the work of a Chemistry Institute, where research was organised on lines which the operation of the Government scheme will make general. If, from the very nature of things, my experience cannot be lengthy it is at least intimate, and I may perhaps be allowed to lay before you my impressions of the problems we have to face.

Two main objectives lie before us: the expansion of useful learning and the diffusion of research experience among a selected class. This class in itself will form a new unit in the scientific community, and from it will emerge the 'exceptional man' to whom, quoting Sir James Dewar, 'we owe our reputation and no small part of our prosperity.' When these words were uttered in 1902 it was a true saying that 'for such men we have to wait upon the will of Heaven.' It is still true, but there is no longer the same risk that the exceptional man will fall by the way through lack of means. Many types of the exceptional man will be forthcoming, and you must not imagine that I am regarding him merely as one who will occupy a University Chair. He will be found more frequently in industry, where his function will be to hand on the ideas inspired by his genius to the ordinary investigator.

I have no intention of wearying you by elaborating my views on the training required to produce these different types. My task is greatly simplified if you will agree that the first step must be systematic experience in pure and disinterested research, without any reference to the more complicated problems of applied science. This is necessary, for if our technical research is to progress on sound lines the foundations must be truly laid. I have no doubt as to the prosperity of scientific industries in this country so long as we avoid hasty and premature specialisation in those who control them. We may take it that in the future the great majority of expert chemists will pass through a stage in which they make their first acquaintance with the methods of research under supervision and guidance. The movement is already in progress. The Government grants are awarded generously and widely. The conditions attached are moderate and reasonable, and there is a rush to chemical research in our colleges. Here, then, I

issue my first note of warning, and it is to the professors. It is an easy matter to nominate a research student; a research laboratory comfortably filled with workers is an inspiring sight, but there are few more harassing duties than those which involve the direction of young research chemists. No matter how great their enthusiasm and abilities, these pupils have to be trained, guided, inspired, and this help can come only from the man of mature years and experience. I am well aware that scorn has been poured on the idea that research requires training. No doubt the word is an expression of intellectual freedom, but I have seen too many good investigators spoiled and discouraged through lack of this help to hold any other opinion than that training is necessary. I remember, too, years when I wandered more or less aimlessly down the by-paths of pointless inquiries, and I then learned to realise the necessity of economising the time and effort of others.

The duties of such a supervisor cannot be light. He must possess versatility; for although a 'research school' will doubtless preserve one particular type of problem as its main feature, there must be a sufficient variety of topics if narrow specialisation is to be avoided. Remember, also, that there can be no formal course of instruction suitable for groups of students, no common course applicable to all pupils and all inquiries. Individual attention is the first necessity, and the educative value of early researches is largely derived from the daily consultations at the laboratory bench or in the library. The responsibility of becoming a research supervisor is great, and, even with the best of good will, many find it difficult to enter sympathetically into the mental position of the beginner. An unexpected result is obtained, an analysis fails to agree, and the supervisor, out of his long experience, can explain the anomaly at once, and generally does so. If the pupil is to derive any real benefit from his difficulties, his adviser must for the moment place himself in the position of one equally puzzled, and must lead his collaborator to sum up the evidence and arrive at the correct conclusion for himself. The policy thus outlined is, I believe, sound, but it makes severe demands on patience, sympathy, and, above all, time.

Research supervision, if conscientiously given, involves the complete absorption of the director's energy and leisure. There is a rich reward in seeing pupils develop as independent thinkers and workers, but the supervisor has to pay the price of seeing his own research output fade away. He will have more conjoint papers, but fewer individual publications, and limitations will be placed on the nature of his work by the restricted technique of his pupils.

I have defined a high standard, almost an ideal, but there is, of course, the easy alternative to use the technical skill of the graduate to carry out the more laborious and mechanical parts of one's own researches, to regard these young workers as so many extra pairs of hands. I need not elaborate the outcome of such a policy.

There is another temptation, and that, in an institution of university rank, is for the professor to leave research training in the hands of his lecturers, selecting as his collaborators only those workers who have passed the apprenticeship stage. This, I am convinced, is a

mistake. Nothing consolidates a research school more firmly than the feeling that all who labour in its interests are recognised by having assigned to them collaborators of real ability.

I am not yet done with the professor and his staff, for they will have other matters to attend to if research schools are to justify their existence and to do more than add to the bulk of our journals. In many cases it will be found that the most gifted of the young workers under their care lack what, for want of a better expression, is known as 'general culture.' Remember, these graduates have just emerged from a period of intensive study in which chemistry and the allied sciences have absorbed most of their attention. For their own sake and in the interests of our subject, they must be protected from the criticism that a scientific education is limited in outlook and leads to a narrow specialism The research years are plastic years, and many opportunities may be found in the course of the daily consultations 'to impress upon the student that there is literature other than the records of scientific papers, and music beyond the range of student songs.' I mention only two of the many things which may be added to elevate and refine the research student's life. Others will at once occur to you, but I turn to an entirely different feature of research training, for which I make a special plea: I refer to the inculcation of business-like methods. You will not accuse me, I hope, of departing from the spirit of scholarship or of descending into petty detail, but my experience has been that research students require firm handling. Emancipated as they are from the restrictions of undergraduate study, the idea seems to prevail that these workers ought to be excused the rules which usually govern a teaching laboratory, and may therefore work in any manner they choose. It requires, in fact, the force of a personal example to demonstrate to them that research work can be carried out with all the neatness and care demanded by quantitative analysis. Again, in the exercise of their new freedom young collaborators are inclined to neglect recording their results in a manner which secures a permanent record and is of use to the senior collaborator. As a rule, the compilation of results for publication is not done by the experimenter, and a somewhat elaborate system of records has to be devised. It should be possible, twenty years after the work has been done, to quote the reasons which led to the initiation of each experiment, and to trace the source and history of each specimen analysed, or upon which standard physical constants have been determined. I need not enter into detail in this connection beyond stating that, although a system which secures these objects has for many years been adopted in St. Andrews, constant effort is required to maintain the standard.

One of the greatest anxieties of the research supervisor is, however, the avoidance of extravagance and waste. The student is sometimes inclined to assume a lordly attitude and to regard such matters as the systematic recovery of solvents beneath his notice. My view is that, as a matter of discipline as much as in the interests of economy, extravagant working should not be tolerated. There is naturally an economic limit where the time spent in such economies exceeds in

value the materials saved, and a correct balance must be adjusted. It is often instructive to lay before a research worker an estimate of the cost of an investigation in which these factors of time and material are taken into account. As a general rule it will be found that the saving of material is of greater moment than the loss of time. The point may not be vitally important in the academic laboratory, but in the factory, to which most of these workers eventually migrate, they will soon have the lesson thrust upon them that their time and salary bear a small proportion to costs of production.

You will see I have changed my warning from the professor to the student. A student generation is short. In a few years, when almost as a matter of course the best of young chemists will qualify for the Doctor of Philosophy degree, it will be forgotten that these facilities have come to us, not as a right, but as a privilege. Those who reap the advantages of these privileges must prove that the efforts made on their behalf have been worth while.

Looking at the position broadly, if one may criticise the research schemes of to-day, it is in the sense that the main bulk of support is afforded to the research apprentice, and the situation has become infinitely harder for the supervisor in that new and onerous tasks are imposed upon him. To expect him to undertake his normal duties and, as a voluntary act, the additional burden of research training is to force him into the devastation of late hours and overwork. The question is at once raised—Are we using our mature research material to the best advantage, and is our policy sufficiently focussed on the requirements of the experienced investigator? I think it will generally be agreed that members of the professor or lecturer class who join in the movement must be relieved in great measure of teaching and administrative work. I am decidedly of the opinion that the research supervisor must be a teacher, and must mingle freely with undergraduates, so as to recognise at the earliest possible stage the potential investigators of the future and guide their studies. To meet this necessity universities and colleges must realise that their curriculum has been extended and that staffs must be enlarged accordingly. There could then be definite periods of freedom from official duties for those who undertake research training as an added task. Opportunities must also be given to these 'exceptional men' to travel occasionally to other centres and refresh themselves in the company of kindred workers. It is evident that our universities are called upon to share the financial burden involved in a National Research scheme to a much greater extent than possibly they know.

I may perhaps summarise some of the conclusions I have reached in thinking over these questions. The first and most important is that in each institution there should be a Board or Standing Committee entrusted with the supervision of research. The functions of such a body would be widely varied and would include:—

1. The allocation of money voted specifically from university or college funds for research expenses.

2. The power to recommend additions to the Teaching Staff in departments actively engaged in research.

3. The recommendation of promotions on the basis of research achievement.

4. The supervision of regulations governing higher degrees.

Among the more specific problems which confront this Board are the following:—

1. The creation of Research Libraries where reference works can be consulted immediately.

2. The provision of publication grants, so that where no periodical literature is available the work will not remain buried or obscure.

3. The allocation of travelling grants to enable workers to visit libraries, to inspect manufacturing processes, and to attend meetings of the scientific societies.

I have dealt merely with the fringe of the question, but would add that there is one thing which a Research Board should avoid.

It is, I am convinced, a mistake for a governing body to call for an annual list of publications from research laboratories. Nothing could be more injurious to the true atmosphere of research than the feeling of pressure that papers must be published or the Department will be discredited.

What I have said so far may seem largely a recital of new difficulties, but they are not insurmountable, and to overcome them adds a zest to life. It would have taken too long to go more fully into details and I have tried to avoid making my address a research syllabus, merely giving in general terms the impressions gained during the twenty years in which the St. Andrews Research Laboratories have been in existence.

Save for the fact that I realise my audience is not confined to university teachers I would have liked to speak on some such points as these: The choice of a research student, the selection of a research subject, the writing of scientific papers. Each would demand a lengthy discussion, as would also the painful situation created when a research topic fails or a research worker proves disappointing.

I have confined myself to the first stage in the research development of the chemist. His future path may lead him either to the factory or to the lecture-room, and in the end the exceptional man will be found in the director's laboratory or in the professor's chair. However difficult these roads may prove, I feel that with the financial aid now available, supported by the self-sacrificing labours of those who devote themselves to furthering this work, he has the opportunity to reach the goal. It is the beginning of a new scientific age, and we may look forward confidently to the time when there will be no lack of trained scientific intellects to lead our policy and direct our efforts in all that concerns the welfare of the country.

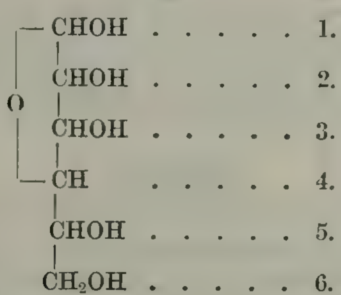
SOME RESEARCH PROBLEMS IN THE CARBOHYDRATES.

ADDRESS (PART II) TO SECTION B (CHEMISTRY) BY
PRINCIPAL J. C. IRVINE, C.B.E., D.Sc., LL.D., F.R.S.,
PRESIDENT OF THE SECTION.

CELLULOSE, STARCH, AND INULIN.

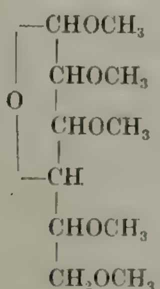
In submitting at this stage an account of the researches upon which my co-workers are at present engaged, I am impressed by the recollection that the first paper on the alkylation of sugars was read to Section B twenty years ago. The communication[1] dealt merely with the progressive methylation of methylglucoside and with the trimethyl and tetramethyl glucoses to which the products give rise. Even at that time it was recognised that the study of methylated sugars opened up a new method of attacking the constitutional problems of the carbohydrates, and the further progress reported to the Association in the following year showed how the process could be applied to determine, in part, the structure of sucrose and maltose.[2] The principle underlying these studies may be very briefly stated. Adopting for the moment the accepted formula for glucose it will be seen that a hexose sugar contains five hydroxyl groups:—

I.

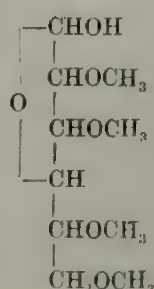


It is to be noted that one of these groups differs from the remaining four in that, although it may be replaced by a methyl group, this is easily removed by acid hydrolysis. On the other hand, when methyl groups are introduced into the remaining positions (numbered 2, 3, 5, and 6 in the formula), they are exceedingly resistent to hydrolytic action. It follows, therefore, that a fully methylated glucoside (II.) when heated with acid will be converted into a tetramethyl glucose (III.).

II.



III.



Similar reactions are impossible with acetyl or benzoyl derivatives of sugars owing to the ease with which the substituting groups are eliminated. In order to illustrate the utility of methylation in determining structure, we may ascribe to any sugar derivative the general formula S—G, where S is a sugar residue and G the group with which it is condensed. Complete methylation may be effected, according to the solubilities involved, by silver oxide and methyl iodide or, alternatively, by methyl sulphate and alkali. The compound obtained will yield, on hydrolysis, at least two products, one of which is a methylated sugar. Determination of the number and distribution of the methyl groups in each of the cleavage products gives the structure of the parent compound, as the mode of attachment of the constituents is thereby known.

In the special case where the group G is also a sugar residue the general formula of the complex may be written $S—S_1$. The compound would thus be a disaccharide, and precisely the same structural study can be applied to it. The method is equally applicable to trisaccharides, $S—S_1—S_2$, and finally to polysaccharides $S \ldots\ldots S_n$.

The development of this line of research has demanded the preparation of a large variety of methylated sugars, which play the part of reference compounds in that the position of the alkyl groups in them is known.

As I wish to deal particularly with the constitutional problems of polysaccharides, I shall make no attempt to summarise the results which have been obtained in the study of the simple sugars, the glucosides, or the disaccharides, but turn at once to the case presented by cellulose.

Cellulose.

(With Dr. W. S. Denham *and* Dr. E. L. Hirst.)

The extensive literature which has grown up on the constitution of cellulose affords little satisfaction to the organic chemist. Despite the complications involved and the many liabilities to error, it seems impossible for workers on cellulose to resist the temptation to ascribe a molecular formula to the compound. The difficulties which stand in the way are too numerous to mention, but the marked stability of cellulose under some conditions and its curious reactivity under others, coupled with its limited solubility and lack of volatility, are outstanding obstacles. As a result, many divergent and even conflicting suggestions have been put forward to represent the polysaccharide structurally, and the views of chemists both within and without the large circle of workers in this field are chaotic. The confusion is increased by the fact that many formulæ for the compound are published in haste to be corrected at leisure, and this unhappy state of affairs was never more pronounced than to-day. I would refer you to an article by Hans Pringsheim,[3] in which he classifies the mentality of investigators and puts forward a plea that, if sure progress is to be made, the chemistry of polysaccharides must be pursued slowly step by step. Impetuous and hasty theorising does infinite harm.

The first essential in arriving at a satisfactory formula for cellulose

is to ascertain if the aggregate $(C_6H_{10}O_5)_n$ is composed entirely of glucose units, as even this fundamental point has been disputed. To obtain the necessary evidence is not so simple as might appear, and many of the 'proofs' which have been offered do not carry conviction to those familiar with the detailed chemistry of the simple sugars. By converting cellulose into the triacetate and thereafter decomposing this product so as to give methylglucoside, we have recently obtained data which leave no doubt that glucose alone is the basis of cellulose.[4] The yield of pure crystalline methylglucoside thus isolated amounts to more than 95 per cent. of the theoretical yield calculated on the basis of the equation:—

$$(C_6H_{10}O_5)_x + x\,H_2O \longrightarrow x\,C_6H_{12}O_6.$$

This result applies only to cotton cellulose, and further discussion is therefore restricted to this particular variety. Disregarding structures which are not based upon glucose, the numerous formulæ proposed for cellulose may be approximately divided into two classes:—

1. Constitutions modelled on that of the glucosides, involving the addition of numerous glucose residues by mutual condensation. According to this view, cellulose consists of large molecules.

2. The unit of cellulose may be regarded as a simple anhydroglucose, $C_6H_{10}O_5$, highly polymerised.

As pointed out, the situation alters almost from day to day, but for the moment a compromise between the above classes is supported, and some authorities prefer to regard cellulose as a simple anhydro-*n*-saccharide (where *n* is a small multiple) polymerised in unknown numbers.

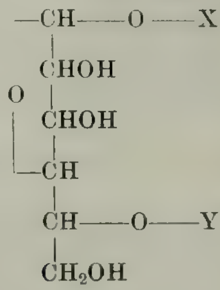
Twelve years ago, after developing the methylation process into a trustworthy method for determining the linkages of sugar complexes, we turned our attention to the constitution of cellulose. The work was undertaken by Dr. W. S. Denham,[5] who, using methyl sulphate and sodium hydroxide as the alkylating reagents, obtained a methylated cellulose in which the methoxyl content was 25 per cent. This value is lower than that required for a dimethyl cellulose (32.6 per cent.), and it followed that, on hydrolysing the product, a mixture of methylated glucoses resulted. From the mixture one definite sugar was isolated, and this Denham[6] proved to be 2,3,6-trimethyl glucose (IV.), which was then isolated for the first time.

IV.

```
  —CHOH
    |
    CHOCH3
 O  |
    CHOCH3
    |
  —CH
    |
    CHOH
    |
    CH2OCH3
```

Denham's work thus gave the first clear evidence as to the linkage of part of the cellulose molecule, and is one of the most important contributions made to the structural study of complex carbohydrates. Cellulose must contain the unit

V.



but as an incompletely methylated cellulose was employed in the hydrolysis the research left unexplained the nature of the residues X and Y.

The investigation was therefore continued with the object of completing the methylation of cellulose and providing answers to the following questions:—

(*a*) Does trimethyl cellulose give, on hydrolysis, a mixture of methylated glucoses in which the average methoxyl content is three groups per C_6 unit?

Or alternatively,

(*b*) If trimethyl cellulose gives trimethyl glucose alone, is this sugar a single individual or a mixture of isomerides?

The War interfered with the progress of the work, but other authors have not hesitated to propose formulæ for cellulose based on Denham's results before he had an opportunity to complete his researches and provide answers to the fundamental questions raised above.

In consultation with Dr. Denham we have repeated his experiments, and amplified them, so that we are now in a position to propose a structure for the cellulose unit which is based on secure evidence. We find that the exhaustive methylation of the polysaccharide, when repeated twenty times, gives a product containing 43.0 per cent. of methoxyl in place of the 45.6 per cent. required for a trimethyl derivative. The carbon and hydrogen values also agree with the formula $(C_6H_7O_2(OMe)_3)_x$, and as the material preserved a fibrous structure there seems little likelihood that profound molecular alteration had taken place. The trimethyl cellulose was heated with a large excess of methyl alcohol containing 1 per cent. of hydrogen chloride for fifty hours at 125-130°. This treatment effected depolymerisation, hydrolysis, and conversion of the scission products into the corresponding methylglucosides. These were distilled in a high vacuum, the total yield obtained being 90 per cent. of the theoretical amount. The following fractions were collected:—

A. Trimethyl methylglucoside.
B. Trimethyl methylglucoside.
C. Trimethyl methylglucoside containing a small proportion of dimethyl methylglucoside.

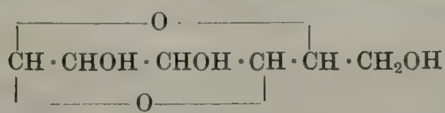
All the fractions were analysed, and in this way it was shown that the small quantity of dimethyl methylglucoside in fraction C agreed exactly with the deficiency of 2.6 per cent. in the methoxyl content of the trimethyl cellulose used. *No trace of tetramethyl methylglucoside was present.* Moreover, the physical constants of the trimethyl methylglucoside agreed exactly with those recently established for this compound by Irvine and Hirst.[7] On hydrolysis of fractions A and B an 89 per cent. yield of crystalline 2,3,6-trimethyl glucose was obtained. The identity of this product was confirmed by analysis, by mixed melting-point with an authentic specimen, and by the mutarotation in aqueous solution

$$[\alpha]_D +108^\circ \longrightarrow +67.0^\circ.$$

No isomeric trimethyl glucose was present; higher and lower methylated glucoses were absent. We thus reach the conclusion that trimethyl cellulose gives 2,3,6-trimethyl glucose as the only product. The reactions involved in the research are shown below, and, considering the nature of the operations involved, the yields may be claimed to be quantitative.

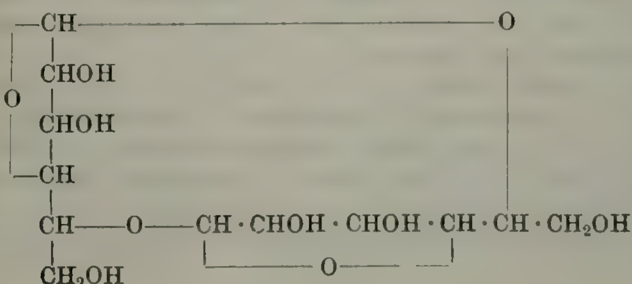
Cellulose
↓
Trimethyl cellulose
↓ } **Yield 90%**
2,3,6-Trimethyl methylglucoside
↓ } **Yield 89%**
2,3,6-Trimethyl glucose

The scheme affords a proof that all the glucose residues in α-cellulose are identical in structure, and the simplest possible formula which will satisfy this condition is that of a 1,5-anhydro-glucose.



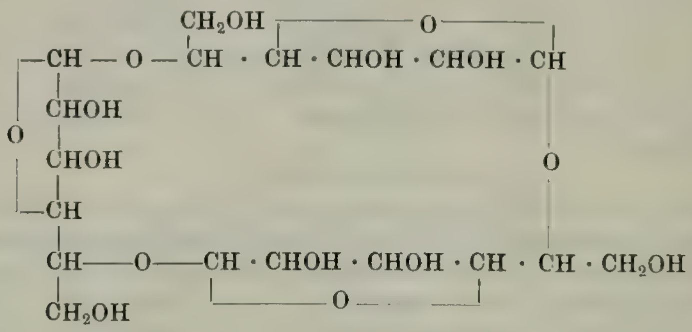
VI.

It is necessary, however, to include at least one additional glucose unit to account for the formation of cellobiose,[8] and this is fulfilled by the formula



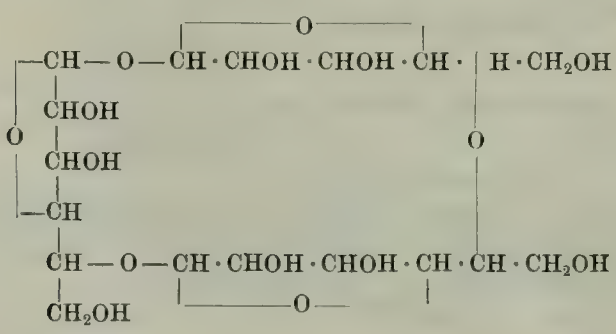
VII.

In terms of the above structure, 100 parts of cellulose should give 105.5 parts of cellobiose, and here the difficulty is encountered that the yields of this disaccharide are extremely variable, and rarely exceed 35 per cent. The highest claimed is of the order 50-60 per cent., and in the meantime it is prudent to select a formula for cellulose which will give a result only slightly higher than this figure. We therefore propose the symmetrical tri-1,5-anhydro-glucose (VIII.) for the unit of cellulose, on the ground that this structure would give a 70 per cent. yield of cellobiose as the theoretical maximum.



VIII.

There is, however, an alternative method of coupling the glucose residues, and this gives the structure shown in Formula IX.

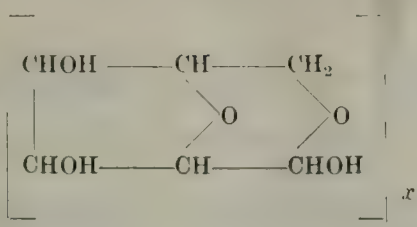


IX.

Taking into account the fact that it can yield only one disaccharide, we prefer Formula VIII. to the above structure. The essential properties of cellulose, so far as they are displayed in chemical reactions, are accounted for by both formulæ. Further, the structures are not inconsistent with the production of bromomethyl furfuraldehyde [9] from cellulose, and indicate that the normal yield of this derivative involves the reaction of one-third of the total unit.

The various formulæ which, in the past, have been proposed for cellulose have been summarised by Hibbert.[10] With the exception of a structure suggested by this author, and in supporting which he prematurely assumed that only one form of trimethyl glucose could be obtained from cellulose, the structures he tabulates do not in any case agree with the evidence we now produce. Typical examples are quoted:—

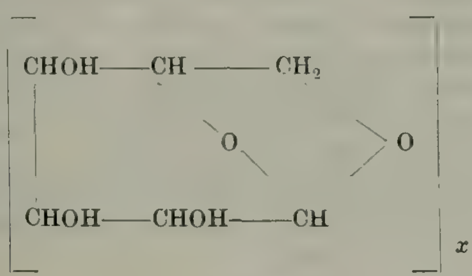
Green's formula[11]:—



X.

would give a trimethyl cellulose which on hydrolysis would lose one methyl group and be converted into 3,4-dimethyl glucose of the amylene-oxide type.

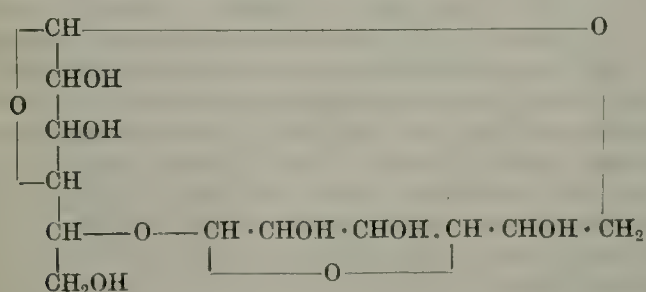
Vignon's formula[12]:—



XI.

is equally unsatisfactory in that the final product should then be 2,3,4-trimethyl glucose of the amylene-oxide type.

The formulæ proposed by Tollens,[13] Cross and Bevan,[14] Bartelemy,[15] and Pictet[16] may be deleted for similar reasons. It is also possible to dispose of Karrer's[17] formula, which is that of an anhydro-cellobiose (termed 'cellosan').



XII.

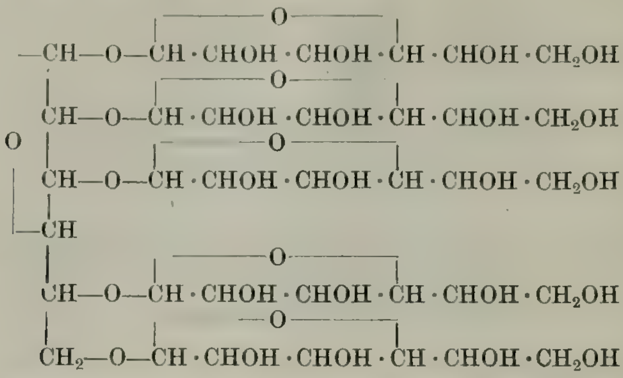
A compound possessing this structure would yield, on methylation and hydrolysis,

1 molecule of 2,3,5-trimethyl glucose
and 1 molecule of 2,3,6-trimethyl glucose.

Our experimental evidence is completely opposed to this view.

An entirely different type of cellulose formula may now be tested. Hess proposes[18] a glucosidic structure which bears a general resemblance to Fischer's constitution for tannins. Space does not

permit of these formulæ being given in full, but the general form will be evident from the simplest example:—



XIII.

Variations are introduced by lengthening the sugar chains until finally a molecule is obtained with eighteen glucose residues ($C_{108}H_{182}O_{91}$). The feature common to all these glucosidic formulæ is that the hydroxyl groups are not symmetrically distributed in the glucose residues. The simplest structure suggested by Hess would give by our processes five molecules of 2,3,5,6-tetramethyl glucose and one molecule of glucose, while his more elaborate molecules would also result in these compounds together with trimethyl glucose. The reactions of trimethyl cellulose exclude all formulæ of this nature.

Starch.

[With Mr. John Macdonald, M.A., B.Sc.]

Turning to the problem of the constitution of starch, we encounter very much the same difficulties as have already been referred to under the heading of cellulose. The importance of the compound, its manifold technical applications, and the special appeal its study makes to the biologist have alike combined to produce a voluminous and somewhat scattered literature. If, however, we eliminate unsupported or contradictory results the following evidence as to structure emerges Starch, when purified from constituents containing nitrogen and phosphorus, possesses the formula $(C_6H_{10}O_5)_x$, and the molecule consists entirely of glucose units. Further, three hydroxyl groups are present for every six carbon atoms, but, as in the case of cellulose, this does not necessarily imply that each glucose residue contains three unsubstituted hydroxyl groups, or that their distribution is symmetrical. The primary reaction of starch, which must be accommodated by a structural formula, is the production of maltose by the action of diastase.

The first essential is the identification of the unit of which the starch molecule is composed, and here, as in the case of other polysaccharides, molecular weight determinations give results of doubtful value. At the present time there is little tendency to regard starch as a highly complex glucoside in which a large number of hexose residues are mutually condensed together, and the view prevails that the polysaccharide is derived from a comparatively simple anhydro-sugar by profound polymerisation. Attention may be focussed on three formulæ based on such ideas.

The unit of starch has been claimed to be:—

1. β-glucosan . . . (Pictet)[19]
2. Anhydro-maltose . . (Karrer)[20]
3. Triamylose . . . (Pringsheim)[21]

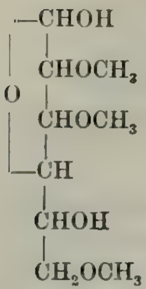
It is possible to test these views by the methylation method.

The first successful methylation of starch was effected by Denham and Woodhouse,[22] and the reaction was afterwards adopted by Karrer.[23] He was, however, unable to complete the alkylation, the substitution being arrested, as we ascertained many years ago, when the methoxyl content was of the order 35 per cent. This value is slightly higher than that demanded for a dimethyl starch, but is considerably lower than that calculated for a trimethyl derivative. It is significant that Pringsheim encountered similar difficulty in methylating the amyloses, diamylose giving a tetramethyl compound, whilst α-tetramylose was converted into the corresponding octamethyl derivative. Pringsheim's combined results lead him to the conclusion that the molecule of starch is built up of not more than 4-6 glucose residues, and, on the whole, he is disposed to retain triamylose as the basis of the polysaccharide.

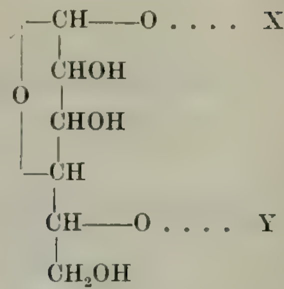
On the other hand, Karrer's view that starch is a polymerised anhydro-maltose rests upon very insecure evidence. The claim that the action of acetyl bromide on the amyloses gives practically quantitative yields of heptacetyl bromo-maltose has been adequately repudiated by Pringsheim. It is, moreover, possible to dispose completely of Karrer's formula for starch by the results now submitted.

When the polysaccharide is methylated repeatedly by the methyl sulphate method, the reaction ceases when the methoxyl content is 37 per cent. It is to be noted that this maximum is not reached when the silver oxide and methyl iodide reaction is employed, as the substitution then stops definitely at the dimethyl stage. Now, the higher value for methoxyl corresponds exactly with the theoretical amount calculated on the basis that one hexose residue has acquired three methyl groups, while four are shared by two glucose residues. Ultimate analysis is also in agreement with this view. Hydrolysis of the methylated starch has shown that this is not a fortuitous coincidence, and we thus obtain a direct clue to the magnitude of the unit which goes to form the starch molecule. When digested with methyl alcohol containing hydrogen chloride the methylated polysaccharide was converted into trimethyl methylglucoside and dimethyl methylglucoside. These were purified by distillation in a high vacuum, and thereafter hydrolysed to give the parent sugars. A totally unexpected result was encountered in that the trimethyl glucose actually isolated proved to be the crystalline form in which the methyl groups occupy the 2,3,6-positions. This sugar has been shown to have the constitution given in Formula XIV., and the linkage of one glucose unit in the starch molecule is thus established (Formula XV.). It is to be noted that this particular type of structure is not present in maltose, but is characteristic of cellobiose and lactose.

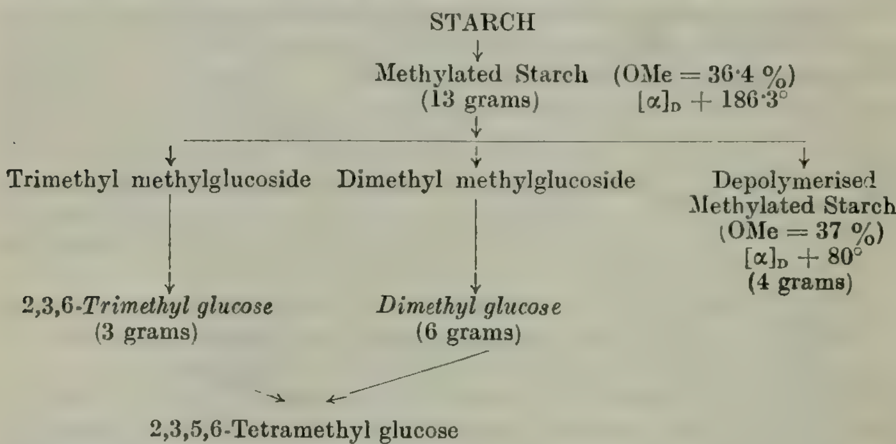
XIV.



XV.

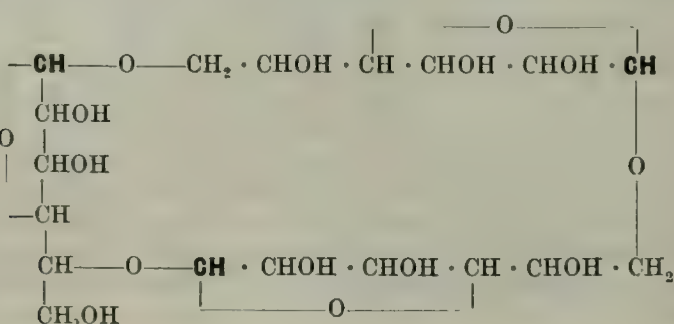


In order to accommodate the formation of maltose from starch either one or two additional glucose residues must be present at X and Y in the unit. Before developing a formula which will fulfil the above conditions an outline of the reactions involved may be given:—

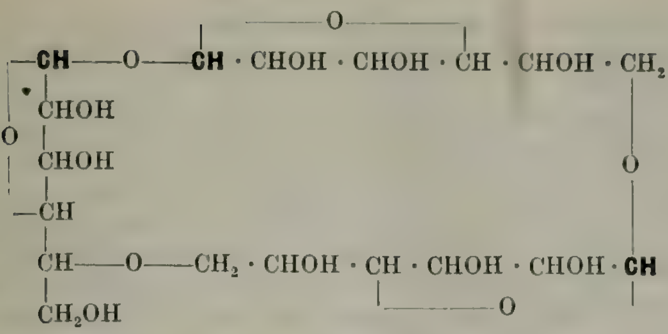


It will be seen that the removal of trimethyl glucose and dimethyl glucose in the molecular ratio of 1:2 is effected without alteration in the *composition* of any methylated starch which survives hydrolysis. The result is striking confirmation of the view that starch is based on an anhydro-trisaccharide in which two hexose residues are linked in a different fashion from the third. One of these is constituted as in Formula XV., and the remaining two must be added in such a way that at least one pair displays the essential structure of maltose.[24]

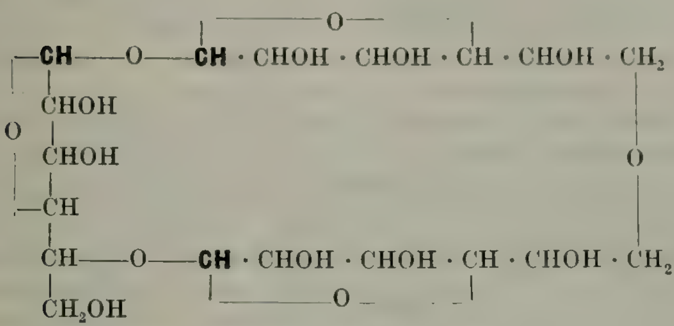
Four different structures may be built up to accommodate these factors:—



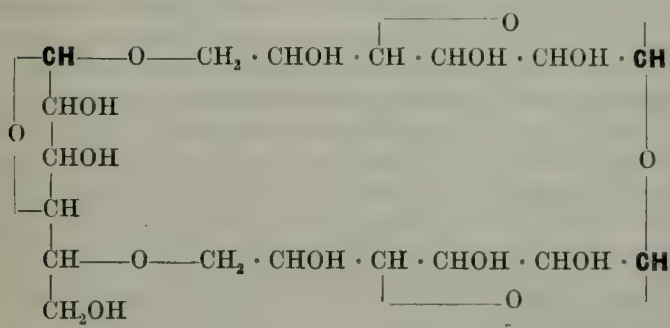
XVI.



XVII.



XVIII.



XIX.

(Letters in block type designate the potential reducing groups.)

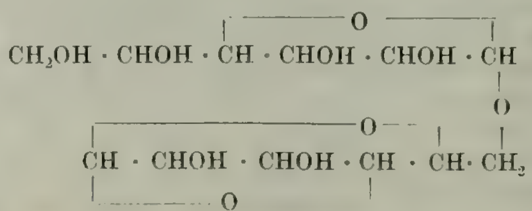
The methylation process cannot discriminate between these possibilities, and with the data at our disposal it is inadvisable to make a definite claim in favour of any one of them. Each formula postulates that starch is derived entirely from the butylene-oxide form of glucose, and this we have shown to be the case. The formulæ are all consistent with the steric hindrance encountered in completing the methylation of starch, previous experience having shown that the alkylation of position 5 is difficult when position 6 of the glucose chain is already substituted.

The formulæ differ in one important respect, as maltose may be obtained from form XVI. in two ways, and in only one way from each of XVII., XVIII., XIX. Pending the completion of further work on this subject we prefer formula XVI., but recognise that our results apply only to a purified rice starch.

One objection may, however, be discussed. In the formation of maltose no more than one molecule of this disaccharide could be obtained from one such unit. The maximum yield of the sugar would therefore be of the order 70 per cent. (74 per cent. calculated as maltose hydrate). Yields higher than this figure are quoted in the literature, but it may be remarked that most specimens of maltose do not behave as identical homogeneous chemical individuals in bacteriological tests. Further, von Euler and Svanberg,[25] who conducted the diastatic hydrolysis of starch under conditions in which the optimum hydrogen ion concentration was present, report that the yield of maltose formed is then 75 per cent. The small margin unexplained by our formulæ may be due to the synthetic action of the enzyme on the molecule of glucose liberated during hydrolysis. This suggestion is in agreement with von Euler's observation that the end point of his reaction was reached with extreme slowness. Another objection to the new structure is that the acetolysis of starch might result in molecular rupture in such a manner that cellobiose would be produced. So far, this disaccharide has not been encountered in the degradation products of starch, a result which is not surprising in view of the uncertainty attending the formation of cellobiose. It may also be mentioned that if starch is composed uniformly of the above anhydro-trisaccharide residues it is difficult to explain the existence of polyamyloses other than those to which the general formula $(C_6H_{10}O_5)_{3n}$ can be applied. Taking into consideration the yield of di- and tetra-amyloses obtained from starch, it is evident that they may possibly be accounted for in the fraction of the starch molecule which has not yet been converted into recognisable glucosides, but the alternative is also open that the polyamyloses may not all be structurally related to starch.

It is perhaps advisable to point out that the experimental results now presented demand the rejection of various formulæ for starch proposed from time to time by Karrer. His structures are based on a diamylose (anhydro-maltose), two formulæ for which have been put forward differing in the position of the anhydro-ring. The evidence he adduces in favour of these views is not convincing.

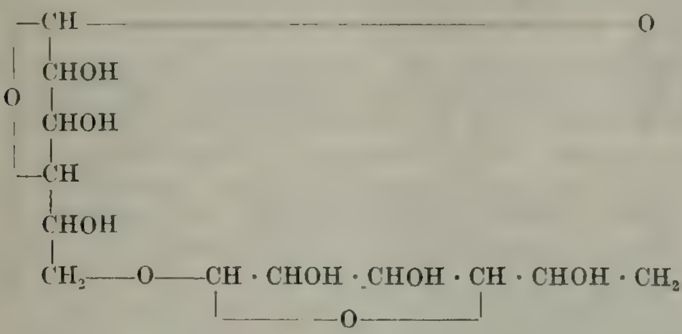
The first unit he gives is:—



XX.

A formula of this type is open to many criticisms, and would demand the direct production of 2,3,5,6-tetramethyl glucose from a methylated starch. It is needless to state further objections, as

Karrer rapidly changed his views in favour of an alternative which is equally incorrect. The unit he prefers at present is:—



XXI.

It is clear that the only trimethyl glucose to which such a structure could give rise is the 2,3,5-form described by Irvine and Oldham.[26] No trace of this compound was detected by us, and, moreover, the 2,3,6-variety of trimethyl glucose actually obtained cannot possibly be accommodated by Karrer's formula. For similar reasons it can no longer be maintained that starch is an aggregate of β-glucosan residues. Irvine and Oldham have proved that glucosan is convertible into 2,3,5-trimethyl glucose, and the same sugar should be formed on hydrolysing methylated starch if there is any structural relationship between glucosan and the polysaccharide. The result obtained is obviously opposed to such a view.

Our work has also thrown light on various other problems connected with the chemistry of starch, including the attachment of nitrogen and phosphorus to the molecule. These elements do not appear to be constituents of extraneous compounds, but form part of the polymerised aggregate. This is shown very clearly by the behaviour of nitrogenous starch, which was methylated in the first instance by the use of methyl sulphate and alkali. Thereafter the product was treated with silver oxide and methyl iodide, but contrary to expectation the whole of the material became converted into an insoluble additive compound with silver iodide. This behaviour does not extend to a purified starch, and finds an exact parallel in the case of glucosamine derivatives which, under identical conditions, form insoluble complexes with silver halides.[27] Obviously if nitrogen were present merely as part of an adventitious impurity only this component would be removed in the course of the silver oxide alkylation, and the fact that the total material was precipitated is a proof that the fragment containing nitrogen is definitely polymerised to the starch unit. Similar considerations apply to the case of glycogen and have served to complicate our work on the alkylation of this compound. It has been established, however, that, as in the case of starch, the methylation of glycogen shows a tendency to be arrested when the methoxyl content is under 40 per cent. The separation of the methylated glucoses is not yet sufficiently far advanced to permit of their identification, but a publication on the exact relationship of glycogen to starch will not be long delayed.

Synthetic Dextrins.

[With Mr. J. W. H. Oldham, B.A.]

With the courteous permission of Professor Pictet we have applied the methylation process to the synthetic dextrins recently prepared by him.[28] These compounds are formed by the polymerisation of β-glucosan and, in properties and composition, they closely resemble the natural dextrins. It is, however, abundantly evident that the synthetic compounds differ structurally from starch as, although methylation was extremely tedious, complete alkylation was effected with the production of trimethyl derivatives. Contrary to expectation, hydrolysis of these compounds did not lead to the formation of 2,3,5-trimethyl glucose, although this sugar has already been obtained from β-glucosan by similar treatment. On the other hand, large quantities of 2,3,5,6-tetramethyl glucose were invariably formed, together with lower methylated glucoses in which the position of the alkyl groups is unknown. It follows from these results that the synthetic dextrins are complex polyglucose-glucosides, and it is interesting to note that they thus conform to the structural type suggested for cellulose by Hess. Although starch is convertible into glucosan and this, in turn, into the synthetic dextrins, it is evident that profound structural alterations must accompany each change.

Inulin.

[With Dr. Ettie S. Steele, Mr. G. McOwan, M.A., B.Sc., and Miss M. I. Shannon, B.Sc.]

The statement has been made that, of all the polysaccharides, inulin is the representative of which the structure has been most definitely established. The opinion is gratifying, but not altogether accurate.

Inulin is derived from fructose, and, until recently, there was no reason to doubt that the parent hexose was the well-known lævo-rotatory form of the ketose. This view is no longer tenable as, although inulin itself yields the normal form of fructose on hydrolysis, trimethyl inulin is converted into a *dextro-rotatory* trimethyl fructose.[29] In similar manner, dimethyl inulin gives a dextro-rotatory dimethyl fructose. Each of these alkylated ketoses was proved to be a derivative of the 'γ-fructose,' which is a constituent of sucrose.

This result places inulin in a position which is quite unique, and the evidence is conclusive that the polysaccharide is entirely composed of γ-fructose residues, each of which retains three hydroxyl groups.

The many problems involved in the constitution of inulin have been discussed by us at some length in recent papers, and attention may now be restricted to the additional evidence we have secured. One important point left unsettled was whether the trimethyl γ-fructose obtained from trimethyl inulin is a single chemical substance or a mixture of isomerides. The question is fundamental, as only in the event of the methylated fructose proving to be a single individual are we justified in claiming that the hydroxyl groups in inulin are symmetrically disposed. The experimental difficulties in the way are

formidable, as methylated fructoses of the γ-type are liquids and give no crystalline derivatives. By the following method, however, it has been possible to obtain the necessary information, and it is now established that only one form of trimethyl fructose is produced from inulin.

A large quantity of trimethyl inulin was digested with methyl alcohol containing hydrogen chloride under conditions which effected:—

(*a*) Depolymerisation,

(*b*) hydrolysis,

(*c*) condensation to give trimethyl methylfructoside.

The product was distilled in a high vacuum, and fractions were abstracted at frequent intervals while the boiling-point remained constant. All the fractions showed the same refractive index and specific rotation. Moreover, the speed of hydrolysis of the fructoside, as indicated by polarimetic observations, was in each case the same, and in each experiment the trimethyl fructose then formed showed identical physical constants. Of greater importance is the fact that all the specimens of trimethyl methyl-fructoside reacted in the same way when dissolved in acetone containing hydrogen chloride. Under these conditions trimethyl fructose monoacetone was formed, and here again the speed of the reaction measured polarimetrically showed no difference in any of the specimens.

There can be no doubt, therefore, that inulin is an aggregate of anhydro γ-fructose residues and that each of the units is identical.

As the exact structure of the methylated fructoses of the γ-type is not yet determined with certainty, it is needless to speculate here on the manner in which these residues are united. The subject has been engaging our attention for a considerable time, but is complicated by the readiness with which the methylated inulins undergo both polymerisation and depolymerisation.

It is not unlikely, bearing in mind the structure assigned to cellulose and starch, that inulin is based on a tri-anhydro-γ-fructose.

The structural discussion which I have had the honour to lay before you on one of the most important groups of natural compounds is admittedly incomplete, and no claim is made that the formulæ now submitted are final. But they at least indicate a new development in the chemistry of polysaccharides, and lines of further research are opened out which promise in time to reveal the intimate constitution of these substances. Much reliance has been placed on the validity of the methylation process as a means of determining structure, but it has to be remembered that most speculation of the kind on carbohydrates is now based on results obtained by this one particular method. The structure of glucosides, the nature of γ-sugars, the constitution of sucrose, maltose, and cellobiose, are all involved in current discussions on this subject, and all are based on the properties of the simple alkylated sugars.

Numerous details, such as the specific reactions of the individual hydroxyl groups in carbohydrate units, have still to be settled before the further problem of the polymerisation of polysaccharides can be

adequately dealt with, but many features, for the most part unexpected, have been revealed.

The polysaccharides, like many other research fields, are, after all, not so complicated as they appeared when viewed from afar, and the close relationship now established between cellulose and starch, starch and lactose, inulin and sucrose, will, it is hoped, play a part in bringing within the range of exact experiment the structural study of all types of natural compounds related to the simple sugars.

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THE PHYSIOGRAPHY OF THE COAL SWAMPS.

ADDRESS TO SECTION C (GEOLOGY) BY

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PRESIDENT OF THE SECTION.

THIS enterprising and progressive city in which we are assembled is one of the three ports of shipment for the products of our most important coalfield and for the entry of many of the commodities which we receive in exchange. It seems not unfitting, therefore, that I should address you upon geological problems relating to Coal, more especially as the development of the portion of the Coalfield concealed under newer rocks is approaching nearer and nearer to Hull.

The subject of Coal Measures Geology has been discussed piecemeal in innumerable papers and memoirs, so that an inquirer may well be appalled at the mass of facts and of often conflicting deductions with which he is confronted. Indeed, it is surprising to discover how fundamental are some differences of opinion which exist.

A cause that has largely contributed to this confusion has been that the geological specialist has commonly worked too exclusively on the outside of the earth, and the miner, who has viewed things below, has seldom attempted any broad generalisations, his experience being limited usually to a small number of collieries or of coal-seams.

In my treatment of the subject I shall be frankly and freely speculative, for I hold that the Geology of Coal has now reached a stage when the mass of accumulated data calls for an attempt at a general review and synthesis. A Scottish Divine, addressing members of the British Association at Edinburgh last year, said: 'An ounce of theory is worth a ton of fact.' With some qualifying adjectives this embodies a profound truth. A carefully considered and weighed theory is worth a great mass of uncoordinated facts, and when I survey the vast undigested, though not indigestible, mass of facts in the body of coaly literature—without taking into account the 250 million tons of solid black facts raised by the British collieries in an average year—I am emboldened to cast into the opposite scale an ounce or so of theory compounded from the ideas of my illustrious fellow-workers and perhaps an odd grain or two of my own.

Growth in Place, or Drift.

Among the questions in the answer to which doctors have differed there is, I imagine, none more fundamental than this:

Were coal-seams simple aggregations of plant remains swept together by the action of water—a process of accumulation which the learned

call allochthony; more simply by drift; or were they formed, like peat, by the growth of vegetable material in its place—the process of autochthony?

I do not intend to labour the answer to this question. Categorical arguments in favour of the growth in place origin of the coal-forming vegetation are on record, and they have never been as categorically answered. Many arguments in favour of the Drift Theory seem to me clearly to have arisen from confusion between cannel and true coal. This distinction is again fundamental. True coal-seams are characterised by:—

(1) Wide extent.
(2) Uniformity of thickness and character over extensive areas.
(3) Freedom from intermingled detrital mineral matter.
(4) Constant presence of a seat-earth or rootlet bed.
(5) Entire absence of remains of aquatic animals within the seam.

Substitute affirmatives for negatives, and negatives for affirmatives, and the characteristics of cannel are as truly set forth. The whole subject has been exhaustively reviewed with all the resources of wide study and great field experience in Professor J. J. Stevenson's memoirs or monographs entitled respectively *The Formation of Coal Beds* and *Inter-relation of Fossil Fuels*, volumes which are treasure-houses of facts. Without a familiar knowledge of these two masterpieces of scientific induction, no geologist is fully equipped for an inquiry into the Geology of Coal. Not the least arresting chapters are those in which the author demonstrates the inadequacy of river-drift to provide materials for the formation of a coal-seam. He shows that even when in high flood the gross amount of timber, drift-wood and general raffle of plant detritus carried along and available for the purpose of coal-seam formation is quite insignificant. He gives many citations from reports of geologists and others, as well as from his own experience, to show that when a flooded river sweeps through a forest it scarcely, if at all, disturbs the humus.

Haigh-Moor or Deltaic Swamp.

Granted, therefore, that coal-seams were, in the main, formed by the growth, death, and accumulation on the spot of plant tissues after the general manner of beds of peat, our next inquiry must be into the further and consequent question: What type of modern peat-growth most nearly represents the conditions of the old peat areas? Were they upland or lowland peats; were they wet or dry? If in search of an answer to this question we examine a section of Coal Measures strata in which coal-seams are included, we find a series of well-stratified layers of sandstones, shales, and the like, exhibiting general regularity of bedding, fine lamination of the layers, and the frequent occurrence of beds charged with the remains of aquatic animals, some marine and some of fresh-water habitat. We cannot fail to recognise in this the inexpugnable evidence of a lowland area undergoing intermittent depression, such as would bring in, at one time, the muds and sands of an area of alluvial drainage, and, at another time, even the sea. We are presented, then, with a clear initial conception of a vast

lowland peat-bog coextensive with, not merely the coal-seam as it now exists, but with its much greater development before denudation had clipped its edges or cut it into several detached areas. This must be our starting-point and principal postulate.

In obedience to the wholesome admonition that the geologist should interpret the past by the present, I have sought in descriptions of the great alluvial areas of the world for some tract that shall exhibit to us conditions closely paralleling—after allowance for biological differences—those of Coal Measure times, especially as regards the extent of the areas of peat formation. In the great Dismal Swamp of Virginia some resemblance may be found, but the area is far too small. The Amazon alluvium is comparable in area, but we have no knowledge of the peats. The deltas of the Nile and the Indus equally fail us. The Ganges delta comes much nearer. But after long flights of inquiry in many parts of the earth I find that one of the best illustrations lies very literally at our doors. At some period subsequent to the Pleistocene Ice Age the whole of the British Isles appears to have stood—relatively to the sea—at the least 80 ft. above its present level, and this uplifted position similarly affected Holland, Belgium, and much of France. The North Sea, in its southern half, appears to have been brought by the net effect of glacial erosion and deposition to the condition of a vast plain so nearly at the then sea-level that it became a morass. Round its margins were forests of oak, pine, and birch, while the greater part of the area furnished the conditions for a great peat-swamp. Under favourable conditions of tide, peat-beds, with or without the frayed and torn stumps of trees in position of growth, may be seen below high-water mark, and in this city of Hull the Forest Beds are exposed when deep cuttings are made or they are encountered in borings. In Holland the peat-beds are similarly present, and in the excavations for docks at Antwerp a peat-bed was found, overlain by a deposit with estuarine shells. This evidence alone would do no more than prove a fringe of swamps surrounding the North Sea, but the trawlers who rake every square mile of the North Sea floor find their operations impeded in places by masses of peat (Moorlog). Clement Reid, to whom we owe much of our knowledge of Post-glacial conditions, expressed his interpretation of the physiography of the North Sea area at this stage in a map which showed the portion of the North Sea south of a line joining Flamborough Head to the northern point of Denmark as a plain intersected by the multiple tributaries and mouths of the Rhine, the Weser, the Elbe, and other rivers.

If we assume that the peat-beds found on the margins and at many stations upon this sea-floor were once approximately continuous, the area would furnish the nearest modern parallel in respect of size to the ancient peat-morasses which the coalfields must once have presented. Beyond this the parallelism fails us. The Coal Measures of England are preponderantly of fresh-water origin, as the recurrent beds of *Carbonicola* and its allies demonstrate, while the Holocene peats of the Dogger Bank and Holland are associated chiefly with marine or estuarine deposits. The coals of the Lower Carboniferous of the North of England and of the Scottish Lowlands, on the other hand, present

frequent intercalations of marine strata—generally of limestone—which is again unlike the North Sea peat-fields.

In the Yorkshire Coalfield—I hope I may be forgiven for styling the coalfield extending from Leeds to Nottingham by the short title 'The Yorkshire Coalfield'—in the Yorkshire Coalfield, then, marine intercalations are represented by beds of shale of extremely fine, blue, unctuous, almost textureless character. These are never more than 20 ft. in thickness, though it would not, perhaps, be quite fair to judge by this relatively small thickness that the marine incursions, though not infrequent, were of proportionately short duration.

The Coal Measures.

The succession of strata in a coalfield exhibits a considerable diversity. Shales—the laminated muds of the old lagoons and swamps—are the predominant elements. These vary much in texture from coarse sandy and micaceous deposits, scarcely separable from actual sandstones, through finer and finer materials to the 'marine bands' already referred to.

The sand—now sandstone—beds are second in bulk only to the shales, and it is interesting to observe that two principal stratigraphic forms are presented. There are, first, the broad sheets extending over hundreds or even thousands of square miles—for example, the Elland Flags are recognisable by their position in the sequence throughout the coalfield from Leeds to Nottingham; their equivalent has been identified in Lancashire and with much probability in North Staffordshire. Whether it reappears in North Wales is doubtful, but its total area must certainly extend to several thousands of square miles. No signs of marine life accompany it anywhere, but in the northern part of its range it is surrounded, either directly or more commonly with an interval of twenty or thirty yards, by a coal-seam—the Better Bed, renowned for its purity and for the very valuable fire-clay that accompanies it. Within or directly upon the top of the rock was found the magnificent stigmarian root now in the Manchester Museum. The evidence, positive and negative, proves the Elland Flags to be of the delta flat type. No decisive indication of the direction of the stream has been sought, though the analogy of the great sandstones of the Millstone Grit series tempts me to conjecture that it was from N. or N.E., and this surmise is strengthened by the occurrence of the coal-seam in that direction and not to S. or S.W., and we may picture a great sandy delta growing by its edge southward and westward with a peat-swamp establishing itself on the higher parts.

Lesser sandstones of the same general types are of frequent occurrence and bear the same general interpretation.

In the other principal stratigraphic form the sandstones are of more limited extension, especially in what I take to be the width of the bed. This type is often called lenticular, but the adjective is a bad one, for such a bed is in fact shaped more like the bean-pod than the bean. Beds of this character I interpret as the infilling of channels cut in the deltaic flats.

Two lithological types peculiar to strata of the Coal Measure facies

are the underclays and gannisters—'seat-earths' to use their common denominator. These are the clayey or sandy beds which underlie coal-seams, or have stood in the relation of soil to vegetation insufficient in amount to form coal. There are many more of these old soils than there are coal-seams.

Both underclays and gannisters possess physical and chemical characteristics which separate them from all other deposits. Physically they are destitute of signs of lamination, and they are traversed by carbonaceous markings often to be identified with certainty as the roots, rootlets, and rhizomes of plants, usually lycopodiaceous. These have pierced through and through the material, obliterating any traces of bedding which may once have been there. The chemical peculiarities of the old soils are the absence of alkalies, the rarity of calcium compounds, and the low percentage of iron. These features also can be ascribed to abstraction of mineral substances by plant activity, and perhaps in part to leaching by passage of water charged with organic compounds arising from the decay of vegetable tissues.

In some beds—but never in the seat-earths—molluscan remains occur. They are of two contrasted groups. Shells of *Carbonicola*, *Anthracomya*, and *Naiadites* are commonly found in packed masses in which, as is commonly the case in fresh-water muds of our day, the lack of variety is compensated for by a great abundance of individual specimens. Beds containing these fossils are usually argillaceous, but they sometimes constitute important beds of ironstone. The marine bands offer the remains of a much more varied fauna—not usually with the same superabundance of individuals. The fossils include *Pterinopecten*, *Pseudamusium*, *Lingula*, *Orthoceras*, goniatites, and many other genera—obviously a marine assemblage. Sometimes the fine blue shales thus characterised are accompanied by hard limestone.

In the bright-coloured measures belonging to the Staffordian division limestones of peculiar texture and contents are found—commonly called Spirorbis Limestones. Their texture is usually smooth and fine; occasionally they enclose angular fragments of a similar limestone, as though a deposit had been shattered *in situ* by some agency, and deposition of like material had then been renewed.

To all these add coal-seams and the series is complete.

The Constituents of Coal-seams.

We must now turn to a more minute examination of the coal itself. Any seam of ordinary house coal—such, for example, as the Silkstone seam—will present us, if we look closely, with three substances, agreeing in a carbonaceous character and in a modicum of combustibility, but otherwise very different. The first constituent is a black, lustrous, dense material—the typical coal. This is disposed in apparent bedding with some appearance of regularity, though an individual layer can rarely be traced as much as three or four feet, and is never continued for many more. It presents in great perfection a close and regular cleavage perpendicular to the bedding. This cleavage is the *cleat*, of which I shall have more to say presently.

The second constituent, which is rarely altogether absent though

more abundant in some seams than in others, is dull in aspect with a brownish tinge, sometimes even being of a deep coffee colour. It is of rougher texture, breaking with a rather hackly fracture. The third material, commonly known as 'mother of coal'—a name innocent of misleading implications, for which the French name 'fusain' is often substituted—is disposed in thin layers between the other constituents. The fusain layers are of weak texture, so that the coal when struck almost invariably splits along one of these. Upon the cleft surface a soft charcoal-like substance is seen, made up apparently of broken fragments. It is this which makes coal dirty to handle.[1]

What is the meaning of these three types of substance constituting the seam? Even with the unaided eye we may gain a clear reply to our question, but the microscope gives a better answer, and discloses many interesting details. The highly lustrous coal was long ago recognised by Dawson as being produced from the bark of trees, and it is common experience that isolated shells of bark with the characteristic external leaf-scars and internal marks of leaf-traces are usually of bright coal. This, however, is probably not quite the whole of the explanation of the bright coal, for I have in my possession specimens in which a larger fraction of the original radius of a trunk than can be ascribed to bark in the most elastic sense of the term is represented by coal of extraordinary brilliance. Again, where coal has undergone great disturbance prior to its 'mineralisation' it is usual to find a large development of bright coal. However, it is a great aid to the interpretation of a seam to know that the long bright streaks do usually represent in some shape the trunks or branches of trees. The dull coal may be a felt of the finer elements of plants, or, in Lomax's phrase, mixed humic debris, but few, if any, of the seams in the Yorkshire Coalfield fail to include layers of which the main constituents are the spores of lycopods—both megaspores and microspores. With a glass of even low magnifying power we may recognise in hand specimens of coal the small discs which represent the flattened spores, each with a triradiate mark indicating its contact with the other three members of the tetrad. In thin section for the microscope they appear as yellow discs, or sacs, sometimes in horizontal sections showing the three-rayed ridge. The methods by which these plant-spores have been accumulated may have differed. The spores may have been wafted from distant jungles of *Sigillaria* or kindred trees, though their large size is rather opposed to the view of wind carriage from a considerable distance, and it seems the more probable supposition that they accumulated on the ground or on the carpet of vegetation in the water beneath

[1] Dr. Stopes has proposed (*Proc. R.S.*, Ser. B., xc., p. 470) a classification of constituents of coal-seams into four types: 1, Fusain, the familiar 'mother of coal'; 2, Durain, which includes the spore coals (generally these belong to the 'hards' in a seam); 3, Clarain, which apparently includes the humic coal of Lomax; and 4, Vitrain, a very brilliant coal forming thin bands and showing a complete absence of structure in typical specimens. Mr. Sinnatt (*Trans. Inst. Min. Eng.*, vol. lxiii., p. 307) adopts these names and proposes a system of conventional shading by means of which they may be distinguished in drawings. He also attempts an analysis of the proportions of each type in certain coal-seams in Lancashire.

the parent trees. That they were drifted into place and distributed with the thinness and regularity which we see the layers to possess is quite inconceivable. For sometimes a layer of spore coal an inch in thickness may be traced at a specific level in a seam over an area of scores of square miles. In the Haigh Moor seam there is a layer of this kind half or three-quarters of an inch in thickness, which can be traced through several collieries in the neighbourhood of Castleford.

The constituents of fusain, or 'mother of coal,' are even more easily recognised than those of spore coal. Upon a bedding plane fusain is seen to be composed of fragments of plant-tissue, commonly showing a fibrous or cellular structure, and in many instances of rectangular form suggesting scraps of wood. In the seams most commonly used as house-coal in Yorkshire recognisable fragments of Calamite stems are very common—usually in single internodes or lesser fragments, though occasional examples of three or four internodes in apposition are found. Some fusain, according to White, is composed of fern leaves.

The charcoal-like aspect is in agreement with the results of chemical analysis, which show a very high carbon content.

Fusain layers are much less defined in the spore coal than in soft coal, a fact which may have some bearing upon the mode of origin of the two materials. In bright coal the fusain layers exhibit considerable regularity and continuity.

There has been much speculation regarding the origin of fusain layers, some authors ascribing them to the wood and smaller plant rubbish which appear to have undergone rapid aerial decay at or near the water surface of the swamp in which most of the debris was submerged.

This explanation appears to me the most in accord with the facts as I have observed them, but the regularity of the layer seems too great and the fusainisation too indiscriminate and too complete to accord with any supposition that these layers represent the ordinary crop of decaying materials. It would be worth a detailed and systematic study to ascertain whether they represent the raffle of dead twigs, leaves, and other stuff brought down by periodic flood-waters. This supposition gains a little support in my experience of the abundance of calamitean stems, for although *Calamites* is provided with a stout woody axis, the cortex has very large air-spaces that would impart great buoyance to the fragments. I have collected the drift along the flood-line of two English lakes, Bassenthwaite and Semmer Water, and in both cases fragments of *Equisetum* were preponderant elements. Periodic flooding is not inconsistent with what is known about the conditions of coal-formation, or of the régime of great rivers. The great swamps of the world are in the flat portions of the course of great rivers or in their actual delta. The North Sea, for instance, we have chosen for example, was a great deltaic flat. That the Coal Measures were a similar deltaic flat is evident.

The idea that fusain is the imperfectly burnt residue of a forest fire is opposed by so great an array of facts that it is difficult to understand its frequent restatement. The fusain layers are as even and regular

as any of the layers, and may recur several times in an inch of coal. It would be difficult to imagine reafforestation so frequent and so necessarily extensive.

These varying types of material recurrent in the thickness of the coal-seams leave us, it is true, some unsolved problems, but they present us with a sufficient basis of fact to enable imagination to call up the general conditions of coal-formation.

Let us now imagine a great expanse of newly formed or forming mud or sand flats. Over this area semi-aquatic plants creep out and establish themselves, their dead remains and windfalls gradually accumulating into a bed of decaying vegetable debris upon which other plants—not necessarily of the same type—follow. With varying and perhaps recurrent conditions of drainage and moisture one flora succeeds another. Some day it should be possible to map out the ecology of the coalfields at the time of the formation of the coal-seams in somewhat the same way that Dr. W. G. Smith has portrayed the distribution of plant associations on the surface of Yorkshire to-day, and we may be able to trace the chronological plant-sequence, as has been done for modern peat-bogs. This result will be achieved through the study with the microscope of thin sections of coal—especially serial sections extending from base to summit of a particular seam. Such a method of study was first attempted by Wethered, but it was not until mechanical methods of section-making were brought to perfection by Mr. James Lomax that complete success was attained.

There are now available for study—thanks to the interest taken in these inquiries by coal-owners in the Yorkshire Coalfield—six complete series of sections taken from our great Barnsley Bed at geographical intervals of about four or five miles. When the whole coalfield is spanned by a suite of such series a great addition will be made to our knowledge of the swamp-forests of the Coal Measures.

Lomax declares that there is a general succession of constituents recognisable in many seams, which must be related to the predilections of the plants concerned in the matter of drainage and other factors. He says:

'Usually the lower part of a seam consists of a bed of very fine humus or mixture of leaf-like matter, with here and there portions of stems, fructiferous organs, &c., probably derived from the remains of small, more or less delicate, plants, and forming soft bright-looking beds of coal. Ascending upwards in the seam other plant remains are to be found, some belonging to the Gymnosperms.

'Other remains are the Lycopods (Club-mosses), which as time went on increased both in size and vigour, ultimately crowding out almost every other kind of vegetation, and becoming the predominant plants of their time.

'The various changes, progress, and deterioration can be traced until ultimately the plant life represented in the top of the seam is found to be practically identical with that at the bottom.'

Some such sequence is, of course, to be expected. When a bed of mud, sand, or limestone emerges from below water-level to be a land surface it could not be expected that every type of plant-life could

grow upon it without preparation. And Lomax remarks: 'In order to prepare a humus for the higher plants humic material must have accumulated by the growth of lower orders.'

The general result of Lomax's studies—in which result my experience enables me to concur—is that the base of a seam is a rather soft coal, exhibiting upon a vertical face a dull ground mass with fine spindle-shaped streaks of bright, lustrous coal, apparently composed of small scraps of a variety of plants of what the modern gardener would term the herbaceous type. Following this we have the appearance of a bright coal with sections of compressed stems or branches of trees intermingled with 'humic' material and spores. In the upper part of the seam in general hard coals often occur, consisting mainly, or even exclusively, of megaspores and microspores, with an occasional sporangium, or even a complete fruit.

This is the simple succession. There may, however, be a recurrence of any of these phases. At first inspection the sequence seems to fortify Lomax's inference that the giant Lycopods demanded a soil of humic materials upon which to grow, but this inference must admit of many exceptions to meet the innumerable cases of fossil-trees standing rooted in sandstone (gannister), or other purely mineral deposit, with no trace of humic soil. I have also seen a two-inch seam with its underclay resting upon a coralliferous limestone into which the stigmarian roots had penetrated.

The nature of the last crop on the ground is not infrequently indicated by the plant-remains in the roof. In the coalfield nearest us the most common occurrence is to find in the shales of the roof prostrate stems of *Sigillaria*, very often in great numbers. Not infrequently the mud-filled stumps forming the dreaded 'pot-holes' stand in attitude of growth in the roof shales; their roots, too, may sometimes be detected ramifying in the seam or on its surface. The great Barnsley Bed is sometimes in this condition, but occasionally the last crop of this seam when overwhelmed and drowned in muddy water was a profuse growth of the fern-like Pteridosperms, such as *Neuropteris heterophyllus*. At South Kirkby colliery a whitish efflorescence from the shale with the black carbonaceous plant-remains gives the aspect of a sheet out of a botanist's *hortus siccus*.

Cannel.

I have already mentioned that, in all those characteristics which prove the growth in place origin of true coal, cannel seams present the exact reverse, so that here all authorities are agreed that drifting in some form must be invoked, but there are other forms in which the material occurs to which the general theory can be applied only with some qualification.

The structure and composition of cannel have an important bearing upon all questions of its origin. It is sometimes described as consisting of spores, but in fact all the more exact descriptions speak of a dense amorphous ground mass in which the recognisable structures are usually spores. My observations show that they constitute only a small fraction of the whole. Other plant-remains are rare; indeed, I can recall very few examples, of which the most notable was a calamitean stem of

three or four internodes. But if recognisable plant-remains are scarce, it is far otherwise with remains of animals. Scales, teeth, and bones of fishes are almost invariably present, and it is from cannel that our largest collections of Coal Measure vertebrates have been obtained. Amphibian remains are more rare; Ostracods, such as *Beyrichia arcuata*, are crowded in some planes, and lastly, fresh-water shells such as *Carbonicola* are represented commonly not by the shells themselves, but by the flattened wrinkled epidermis, the calcareous shell having evidently been dissolved by the acids generated by decomposing vegetable matter. The texture of cannel is usually smooth and the fracture conchoidal in the purest specimens, but in most cases it graduates into a black carbonaceous shale. The ash content is always high, rising to 40 per cent. before reaching the point at which it would be regarded as shale. Chemically it is distinguished by the high yield of hydrocarbons, obtained on distillation either as mineral oil or as gas. For this reason, in days before the invention of the incandescent mantle, cannel for enrichment of gas of low illuminating power was in great demand, and commanded so high a price that I have seen our most famous fish-bed worked when it was only seven inches in thickness. All these characteristics of cannel are consistent with the view that it originated from a mass of vegetation macerated in pools of water somewhat after the manner of the 'retting' of flax. Sometimes the cannel is in unconformable relation to the underlying beds, as at the Abram Colliery, Wigan, where it rests in one district upon true coal, and, in the course of about a mile, encroaches first upon the coal, then upon its underclay, and, finally, where seven feet in thickness, it rests upon a bed of shale underlying the underclay. Green suggests that cannel consists of vegetable matter which was drifted down into ponds or lakes and lay soaking until it became reduced to pulp.

Some modes of occurrence of cannel are of particular interest for the light they throw upon Coal Measure conditions. Some beds are of wide extent, having been traced over an area of several hundreds of square miles; on the other hand, strips and patches of a fraction of an acre occur, such as that at the foot of a fault in the Barrow Colliery, which I interpret to indicate a depression in the coal-swamp which was connected with some movement of the fault. An interesting relation is often found to subsist between the total thickness of a coal-seam and the presence of a local patch of cannel. It commonly happens that the presence of a patch of cannel as a constituent of a coal-seam is accompanied by an increased thickness, even out of proportion to the magnitude of the cannel, and this irrespective of whether the cannel is above, within, or below the true coal. It may be explained by the fact that the process of fermentation by which the cannel was produced reduced its volume more rapidly than the ordinary decay did that of the adjacent peat, and so maintained a depression in which more plant debris could accumulate; but the ultimate effect of this fermentation was a less complete loss of hydrocarbons, and consequently, both because its contemporaneous loss was greater and its subsequent loss was less, the presence of a cannel component increases the thickness of a seam.

It may be pointed out that well-decomposed peat forms a buttery mass almost, or perhaps quite, as impervious to water as a bed of clay would be. This may explain why at Teversall Colliery there is a thin bed of poor cannel at the base of the Top Hard (or Barnsley Bed) coal and a second bed of better quality at the top. Where the lower bed is thick the upper one is thin, and *vice versâ*.

Coal-Balls and their Significance.

The bodies known, besides several aliases, as coal-balls are masses of mixed vegetation 'petrified' by being so completely permeated by mineral substances, such as dolomite or calcite, that even the most delicate and tender tissues have been preserved with every cell in its proper position. Coal-balls occur in coal-seams as isolated masses, varying in size from mere pellets up to masses of a ton or two in weight. Sometimes they form clusters closely crowded together and at others sporadically. Apart from their enormous value to palæobotany, they present to the general practitioner in Coal-Measure Geology a number of attractive problems, the solution of which cannot fail to throw a vivid beam of light upon the question of the physiography of the coal-swamps.

Their limitation to seams carrying marine roof-measures at once suggests a source for the petrifying substance and a reason why they are of such restricted occurrence that they are wholly unknown in the great majority of coalfields. The notable memoir by Stopes and Watson [2] is so important a compendium of the significant facts that I shall forbear to cite the writings of others, including myself, who contributed to the discussion. I would further say that I find myself in almost complete agreement with the authors. Their argument in brief is that the seams in question grew in salt or brackish swamps and that a mass of debris of the plants accumulated under water. Sea-water has a remarkable preservative effect upon plant-tissues, experiments by one of the authors showing that fronds of ferns, and even the more delicate structures of liverworts, could be preserved for at least three years without signs of decay or even loss of their green colour. They then proceed to argue that the partial decay of some vegetable materials would liberate carbon dioxide which, reacting with sulphates and sulphides with which the sea-water would have impregnated the mass, produced these isolated concretions which represent a true sample of a bed of peat accumulated on the spot where the plants grew. One instance is cited of two seams separated by a sandstone seat-earth (gannister) in which coal-balls are scattered through both seams. Assuming, as the text implies, that the general character of the concretions is the same throughout the sequence, the inference seems to be justified that the formation of coal-balls was continuing during the whole period of accumulation of the seam. At the same time, it is not clear why the petrifaction should be so local, and it is perhaps worth while to examine any evidence which might decide whether, as happens with some other rocks, the sporadic character may not be due to local escape from decalcification rather than to local petrifaction.

[2] *Phil. Trans.*, Ser. B., vol. 200.

This view of the origin of the seams of coal that enclose these bodies is quite in accord with opinions long held by palæobotanists, that the structure of the plants found in them is compatible with their growth in brackish water, and corroboration is found in the fact that ‘roof-balls’ are found in the overlying shales that contain well-preserved remains of a flora very significantly different from that of the seams.

Boulders in Coal-seams.

The occurrence of well-rounded masses up to several hundredweights of foreign rocks is well attested by many writers, and it is no uncommon occurrence to see small specimens upon the mantelshelf in a colliery office. The subject is, as usual, thoroughly and almost exhaustively summarised by Stevenson,[3] to whose pages any who desire to study the subject further must be referred.

These erratics have been found in coal-seams in many parts of the world. They occur in every part of the seams from roof to floor, and even penetrating the floor. Two forms of transport of these masses have been suggested. The first ascribes it to floating ice, an hypothesis that fails to take account of the smooth, rounded and water-worn appearance of the stones, no less than the incompatibility of ice action on an adequate scale with the climatic conditions indicated, in the judgment of palæobotanists, by the character of the vegetation.

The other explanation, which ascribes the transport to floating trees, is not without difficulty when the size, and particularly the shape, of the boulders is considered. Stevenson comments upon the difficulty of imagining a tree of sufficient magnitude carrying such a load with the tenacious grip which would be required to maintain it from the source of the boulder to its place of deposition. It is clear that thoroughly rounded boulders of intensely hard quartzite could have been shaped only by either a long journey in a mountain torrent or by prolonged pounding on a beach. In either case a tree so burdened would need a considerable depth of water for its flotation, and it is inconceivable that it could steer its way through a forest, unless one deeply submerged. I am disposed to think that such were indeed the conditions—that either during a temporary flood or in the final submergence of the coal-swamp some stray gymnospermous tree whose roots were adapted, as those of *Stigmaria* clearly are not, to wrap round a smooth boulder, drifted over or among the tree tops, and either came to a final anchorage or simply dropped its burden. This explanation is not inconsistent with the presence of boulders at all or any levels in the seam, for it will appear, on reflection, that a mass of rock would readily sink into peat, the rate of its descent being determined by the impetus of its fall, the tenacity of the peat, the shape of the stone, and other factors. Some might bring up against an embedded tree trunk, while others might sink completely through the seam. That some stones have sunk in this manner seems to be indicated by the fact that one of the large stones preserved in the Manchester Museum occupied a vertical attitude in the coal when discovered.

[3] *Op. cit.*, pp. 391 and 426-433.

Surprise is sometimes expressed that these stones should be found in the seams of coal and not in the Coal Measure sandstones and shales that are quarried or wrought in brick-yards. The reason is partly statistical. The weight, and still more the bulk, of these materials extracted year by year is far less than the 250 to 300 millions of tons of coal raised; but a yet more important reason is that no stone in the coal as large as a man's head could escape detection by the collier, and arousing the interest of the officials, whereas in a quarry it would probably, if observed, be cast aside without notice as merely a blemish in the stone of no more interest than any ordinary concretion. The locus of the parent rock of these stray boulders is wholly conjectural, but the great preponderance both in Britain and in America of quartzites should furnish a clue, and the petrologist who will undertake the investigation may certainly rely upon the sympathetic interest of Coal Measure geologists and colliery managers.

The Aberrations of Coal-seams.

Having got our coal-swamp clothed with vegetation, and the coal-forming materials accumulating, let us next consider the various interruptions of continuity and the aberrations to which it is liable. These interferences may be either contemporaneous with the accumulation of the materials, or, as one may say, posthumous. These categories, at first sight, seem capable of easy and definite recognition, but, as we shall see presently, it is not so easy as it looks.

Faults, overthrusts, and unconformities may as a rule be classed among what I have called the posthumous type of interference, though in many cases true faults appear to have achieved a portion of their total movement contemporaneously with the deposition of the seams, or during the interval between seam and seam. An illustration of a contemporaneous fault is found at the Barrow Colliery, near Barnsley, where, on the down-thrown side of the fault and parallel with it, the Thorncliffe Thin Coat swells up from 3 feet to 5 feet 6 inches, and carries a strip of cannel absent elsewhere in the mine. Of a fault moving between seam and seam an example is furnished at Whitwood, where a lower seam is thrown to the extent of 60 feet while an overlying one is unbroken. The case of a fault affecting an upper and not a lower seam is noticed at Aldwarke Colliery.[4] Among the contemporary interferences with the coal-seams are to be accounted unconformities, which, no doubt, occur on various scales of magnitude. Some may be interpreted—as Mr. Clarke suggested for the great 'Symon Fault' of the Forest of Dean—as the denudation of a folded series; other examples would, as I shall presently show, be better explained, as Prestwich explains the Symon Fault, as the erosion of a channel. Prominent in this category of contemporary interferences must be put the phenomena of split seams. A split seam is the intercalation into the midst of the coal of a wedge of sandstone, shale, or the like, in such wise that the seam becomes subdivided by intervening strata into two or more seams. This phenomenon is of special practical importance

[4] *Quart. Jour. Geol. Soc.*, vol. lvii., p. 86.

because it may mean that a thick seam may in the divided condition become incapable of being worked at a profit.

The great coalfield that I have so often cited furnishes examples of every known type, and interesting as they are to the geologist, they are an abomination to the colliery-owner or manager, and often a source of severe disappointment and loss. The most notable split seam in Britain is not, however, in Yorkshire but in the famous Staffordshire Thick Coal. Jukes showed that this magnificent seam, 40 feet thick at its maximum, is split up into a number of minor seams by wedges of sedimentary strata which aggregate, in a distance of 4½ miles, a thickness of 500 feet. Whether these intercalations again thin out, or not, is unknown to me; but whether so, or not, the explanation offered by that sagacious student of coal, Bowman of Manchester, might find here a typical application. Bowman supposed that a local sag occurred in the floor of the coal swamp, resulting in the drowning of the vegetation (in his illustration bearing a suspicious resemblance to a coconut palm) and interrupting the formation of peat until the hollow was silted up and a new swamp flora re-established. This explanation remained for many years unchallenged, but in 1875, in the great memoir on *The Yorkshire Coalfield*, Green advanced a new reason for the splitting of seams, which is a very common phenomenon here, scarcely any, if any, seam being exempt.

Green pointed out that as the Silkstone seam is traced northward from the locality near Barnsley with which its name is associated, it begins to exhibit partings of 'dirt,' which thicken to a belt of country where no collieries afforded information as to the behaviour of the seam. On the far side of this gap a seam is found on the same horizon, but if it represents the Silkstone seam it is very much attenuated and divided. He attributed these features to the development, contemporaneously with the accumulation of the measures, of a ridge of land, whence mud was washed into the coal-swamp on either hand. Later in the same volume exactly the same problem is presented by the Barnsley Bed, which deteriorates in just the same manner in an almost identical geographical position. This was hailed by Green as a further example of the same process.

So long as the problem was of merely academic interest I was content with a silent demurrer, but having to consider the probable resources of the debatable ground for the purpose of colliery development I sought criteria with which to decide whether Green's growing anticline or Bowman's developing syncline was the correct explanation. This was the more necessary as I found that the tendency to split affected seams still higher than those named. Now, it will be obvious upon reflection that an anticline undergoing intermittent elevation and denudation should cause a convergence of the strata representing the stationary phases as they approach the axis, while a deepening trough should produce a corresponding divergence of the strata—principles well illustrated by the Market Weighton and Cleveland axes respectively. A careful plotting of intervals showed that, selecting the two seams that were most generally worked, isopachytes of the strata separating them could be drawn, and Bowman's sag demonstrated.

Care has to be taken in such an inquiry to eliminate a source of error not hitherto taken into account, namely, the relative compressibility of different sedimentary materials. Freshly deposited mud may contain 90 per cent. by volume of water, and even when reduced by time and pressure to the condition of shale may still have 20 per cent. of interspace; a bed of fairly consistent clayey mud might be reduced to one-half its thickness. Sand, however, suffers scarcely any loss of bulk once it has got past the condition of a quicksand. This source of error is eliminated in the calculations relating to the split of the Silkstone and Barnsley seams, and it is seen that the increase of thickness in the sagged area far exceeds the *total* thickness of the sandstone present, so that the sag is a real one and not the effect of the relative compressibility of the measures. There may be cases in which there is no further shore to the sag, and the seam once lost is lost for good and all. Such might be the margin of a deltaic flat undergoing intermittent depression.

It has occurred to me to consider whether the sediments with which the Staffordshire Thick Coal is subdivided need necessarily have demanded an earth movement to an extent corresponding to their aggregate thickness; in other words, whether the aggregate thickness of the sediments plus the seams that they now separate were, in the uncompressed original condition, materially different in thickness from the great undivided seam. I have not the data upon which to found an opinion, but we are promised a full discussion of this seam, when I hope the problem will receive attention. The idea I had in mind has apparently been current for some time, for I find Mr. Walton Brown expressed the opinion many years ago that the Coal Measures might be regarded actually as a single coal-seam, with the necessary implication that the sedimentary measures are in the nature of local interruptions. Some measure of the reduction of thickness which the original substance has undergone and some consequences will be considered later.

I now turn to a form of split seam of extraordinary interest, which has received comparatively little attention from geologists though mining engineers must surely have a special comminatory formula to express their sentiments thereon. The first example that came under my notice was encountered in the eastern workings of the Middleton Main Seam, at Whitwood Colliery, near Wakefield. Thin intercalations of shale and other sedimentary materials, appearing at different horizons in the seam, were found to thicken gradually to the east concurrently with the gradual dwindling of the lower part of the seam. An exploration was then carried out. The bottom coal was followed, but it was found that though the underclay continued the coal disappeared, and was wholly lost for a short distance when it reappeared. The top coal rose over a steadily thickening shale parting, and disappeared into the roof of the workings, but boreholes proved that it was present above a parting which was, at the maximum, 29 feet thick. At the farther end of the heading the top coal came down and the integrity of the seam was restored. Two other transverse explorations have proved the same general arrangement on the same scale of magnitude and one or both margins have been traced for a long distance, enabling the

interruption to be mapped continuously for about 8 or 9 miles and intermittently much further.

My first impression was that this was just a simple case of Bowman's 'sag,' until I observed that in every traverse the *upper element of the seam was arched while the floor was flat.*

Several analogous cases came under my notice before an explanation of this anomalous arching was reached. The explanation was found to lie essentially in the differential shrinkage undergone by peat-stuff in the process of forming coal, and, on the other hand, by any sand or mud which may have been deposited so as to replace a part of the peat.

Let us imagine a stream being diverted at flood time across a bed of peat and scooping out for itself a hollow channel, which channel subsequently becomes filled with sediments, after which the formation of peat continues, the peat plants creep out, and presently envelop the whole mass of sediments. When the beds consolidate there will obviously be very different contraction between the sands, muds, and the coal-stuff. The sands, as I have said, will hardly contract at all, the muds will contract a good deal, the coal-stuff will contract very greatly.

Various estimates—or guesses—have been made of the amount of reduction in bulk which attends the conversion of peat into coal.

Lomax shows that where coal-balls—which are really masses of comparatively uncrushed coal-forming material that has been preserved by minerals infiltering the tissues and the interspaces—occurred abundantly, the seam, including the coal, became thicker according to the quantity of coal-balls present. Where a large number were massed together the seam became more than 6 feet thick, while on every side the coal was not more than a foot thick. Again, he says 'a large mass of petrifactions was found, and which, although more or less crushed by superincumbent weight, retained a height of 7 feet 3 inches, while the corresponding layer of coal was only 10 inches thick.' He estimated the loss by flattening out at one-third 'so that it might be estimated that 11 or 12 feet of vegetable matter had been deposited to form one foot of coal.' [5]

I have found that dry peat can be compressed in a testing machine to one-fifth of its original thickness, and making allowance for the loss in drying, and for the great reduction of bulk attendant upon the change from peat to coal, I am disposed to set a still higher value than Lomax on the reduction. It should be borne in mind that wood has an average of about 50 per cent. of carbon and 50 per cent. of hydrogen, oxygen and nitrogen, while the carbon in an average house-coal ranges from 80 to

[5] Dr. Stopes and Professor D. M. S. Watson adopt a much lower ratio for the compression. They figure a huge coal-ball which 'has entirely replaced the coal-seam where it occurs, leaving but a film of coal at the top and bottom' and it is 'nearly 4 feet thick, while the coal on either side is under 1 foot' (*Phil. Trans.*, B. 200, p. 174). The evidence of this great ball is not at all complete, as not only is there a film of coal of unstated dimensions above and below, but 'streaks of coaly matter run irregularly through it.' Against this may be cited Renault and Zeiller, quoted by Drs. Stopes and R. V. Wheeler. They measured the tracheids in coal and 'other portions preserved uncrushed as a mineralised petrifact. . . . They concluded that the specimen of wood (of *Arthropitus bistriata*) in the coal occupied only one-twelfth of the volume it had in life.'

85 per cent.; but this does not merely imply the loss of 75 or 80 per cent. of the other elements, for the oxygen and hydrogen have gone off largely in combination with carbon. What the gross amount may be I do not venture to say, but my opinion is that the reduction in passing from the state of wet undisturbed peat will not be much less than 15 or 20 to 1.[6]

Let us now, with these facts in mind, return to the consideration of the plano-convex lens of 'dirt' occupying a position between the upper and lower elements of the split seam at Whitwood. On the sag explanation it should be convex downward, yet in this as in all other cases I have investigated, it is convex upward. The explanation is simple. Let us make our mental picture of the infilled channel in the peat a little more specific in detail. Let us suppose that the peat was 40 feet in thickness when the river commenced to cut its course across it; the channel we will say was, like most channels, deeper in the middle than at the sides, and in the middle actually cut through to the seat-earth. Then the channel silted up completely, so that a cast of its meandering course in sands or mud reaching 40 feet in thickness at the maximum, but much thinner at the margins, was formed, then the upper bed of peat formed to a further depth of 40 feet. The conversion of the peat into coal would reduce it to two beds, each, let us say, 2 feet in thickness at the maximum, enclosing the sediment with a proportionately smaller thickness in the eroded peat on either margin of the channel. The sedimentary mass would have the transverse section of a plano-convex lens, the convexity being downward, but when the peat under the edges of the sediment is condensed to one-twentieth of its original bulk the base becomes almost flat, and the unconsolidated mass of sediments adjusts itself thereto. Thus the curve, originally at the base of the mass, reproduces itself in the top of the mass, which was originally quite flat and now is curved. The lens of infilling has reversed its curvature.

In the Castle Comer Coalfield, County Kilkenny, I have been able to examine underground an almost exactly similar case of a portion of a horseshoe-shaped meander exhibiting the same reversal of the lens,

[6] I take this opportunity to expose a fallacy of very wide acceptance. It appears to be a general belief that, as in Coal Measure rocks pebbles of coal occur which are closely embraced by the matrix, and similarly that the shell of coal surrounding a standing tree-trunk is in contact with the matrix both within and without, therefore no appreciable reduction of bulk of the vegetable interior took place in the process of 'coalification.' The assumption here made is that the surrounding rock attained complete induration prior to the accomplishment of that change in the enclosed masses of vegetable matter, yet all analogy forbids that supposition. The Mid-Eocene beds of Alum Bay and Bournemouth, though quite incoherent, contain thin coals as bright and lustrous, as truly 'coalified,' as many of our Carboniferous coals, yet it would hardly be contended that the period that has elapsed since their formation is materially less than the duration of Coal Measure times. The evidence points to the probability that the accomplishment of the greater part of the change from plant to coal took place while the measures were still unconsolidated, and were able to adjust themselves to the shrinkage of the contained masses of coal-stuff. When I come to speak of the cleavage of coal a further argument will emerge in favour of the consolidation of the 'measures' being subsequent to that of the coal.

but in this instance there are additional features of extraordinary interest and significance. The channel is filled mainly with two beds of anthracitic coal, one below and the other above a lens of black shale. The fact that this anthracite is devoid of underclay and that it yields remains of fishes and amphibia at once declares it to have originated as cannel, which I have found to be a usual component of these lenses. Just outside the channel the section at the pit bottom shows 4 inches of coal resting upon an underclay and overlain by coarse sandstone, showing that this is a relic of the original seam, but it must have been largely destroyed by a later incursion of the stream which laid down the sandstone.

The split in the Middleton Main Coal must be regarded as a silted channel of a river that traversed the swamp after the formation of the lower part of the seam, and, as might be expected, evidence is abundant of similar stream action in other phases of the Coal Measure deposition. In the shales intervening between the seams belts of strongly current-bedded sandstone with the transverse section of an infilled trough are often to be found. Small examples are now to be seen in the railway cutting just east of Leeds on the line to Hull; and in Altofts Colliery, Fox Pit, a similar trough has been traced in the roof of the Middleton Main seam for a distance of about half a mile. In this instance it is probable that the direction in which the water was flowing is indicated by the fact that in the N.E. workings the floor of the trough is wholly above the seam, while in the S.W. it is cut into the seam to a depth of about a foot. When a seam is more deeply eroded the only too familiar phenomenon of a 'wash-out,' in the miner's sense, not in that of the modern colloquialism, is formed. We should expect such a deltaic area to afford evidence of the actual meanderings of the main stream, or of its more or less transient tributaries or distributaries. These are most easily recognised by the channels which they cut in the new-formed deposits.

Extensive beds of gravel or conglomerate are of very exceptional occurrence, the source of the materials being in general so remote and the grade of the rivers so low that such deposits would hardly be expected unless the tearing up of the new strata could furnish them—as we shall see that in some cases they did. The lesser 'wash-outs' may be the effects of transient streams which swept across the shallow mud-floored lagoons, cutting out a channel and later silting it up.

Such rivers, contemporary and sub-contemporary with the formation of the coal, show the ordinary complications inseparable from river erosion. They meander on a large scale; the bows are frequently found to be subjected to 'cut-offs,' and in such cases the 'oxbows' frequently contain beds of cannel, speaking of their existence as a stagnant bayou in which vegetable mud accumulated. They exhibit the phenomena of 'cut within cut,' consequent upon rejuvenescence or the scour of flood waters, and the margins are often affected by the falling in of the banks. These are quite ordinary phenomena connotative of the action of moving water.

A typical 'wash-out' occurs in the Parkgate seam at Aldwarke and Rotherham Main Collieries Here a mass of sandstone cuts out

the coal over an area of some hundreds of acres. The sandstone is a strongly current-bedded rock 60 to 80 feet in thickness. Bands of conglomerate, including at times masses of 2 or 3 feet in length, occur. The smaller stones consist of clay ironstone concretions, sometimes with their original form but little modified by attrition. The larger blocks are mostly of sandy shale. Ripple markings are frequent, and large limbs or trunks of trees are encountered. The whole aspect presents a very close resemblance to sections of the old bed of the River Irwell exposed in the cuttings for the Manchester Ship Canal.

This channel must evidently have been that of a river of considerable size which commenced to erode on a plane far above that of the Parkgate seam. This is indicated, not merely by the thickness of the mass, and by the evidence afforded by the pebbles and larger blocks of the erosion of Coal Measure materials, but also it will be noted that the pebbles are chiefly of clay ironstone, betokening a lapse of time sufficient, not only for the deposition of shales, but for the formation of ironstone concretions. This need not, however, have been a very protracted period. The Pleistocene Leda clays of Ottawa contain concretions quite comparable with those now under consideration.

The form of this river channel cannot, at Aldwarke and Rotherham Main, be defined, for the interposition of a few yards of shale would remove it from the ken of the miner except where shafts, or cross-measure drifts to traverse faults, explored the rocks more thoroughly, but it is evident from the records of neighbouring collieries that the Parkgate Rock is not one of the widely extended sandstones of which examples occur in this coalfield, and we may therefore regard the channel which it fills as of limited breadth.

Another instance of the same kind in a seam about 650 feet higher in the Coal Measure series is furnished by the Haigh Moor Rock, which in some places encroaches upon the Haigh Moor Coal seam. It rests upon a conglomerate composed of clay ironstone nodules which, in this instance, can be traced with much probability to their source, for at Robin Hood, about midway between Leeds and Wakefield—where the whole series of strata adjacent to the Haigh Moor seam is exposed in a great excavation, affording one of the best sections of Coal Measures in Yorkshire—the seam is surmounted by a varied suite of rocks comprising coal-seams with their underclays, thin beds of sandstone, and shales containing great numbers of clay ironstone nodules. Such a suite would yield the constituents of the Haigh Moor Rock.

Though it is not practicable to define the course of this rock-filled channel in the way that has been done for the great Warrensburg channel of the Missouri Coalfield, there is yet a convincing proof that it is not an example of folding and denudation, for, if that were the case, the strata would show a diminution as measured from seam to seam as the area is approached, but the area occupied by the rock is just that where the great thickening takes place alluded to *à propos* the splitting of the Barnsley Bed.

An inference of some moment can be drawn from these two eroded channels—general subsidence of the Coal Measure area must have been interrupted at least twice by actual elevation or we should not find

channels cut to the depth of 90 feet in a deposit which must at the time of its deposition have approximated to sea level.

So far we have been examining irregularities of the seam which are clearly connected with the erosive effects of running water. But the majority of the irregularities have not this simple character, and are of a nature quite distinct from the consequences of erosion.

The most common abnormality is the occurrence of belts or patches of 'proud coal' in which the seam swells up to twice or thrice its normal thickness—sometimes, though not always, by repetition of the whole seam or of the upper part, either by shearing or by overfolding. Hull long ago proposed to explain 'proud coal' as the effect of the stony infilling of the wash acting as a wedge of incompressible material forcing out the coal-substance from beneath its margins. I have observed effects attributable to the apposition of coal to sandstone, but they were not of the kind in question.

I have examined, underground, wash-outs in eight different seams, some in only one colliery, others in eight or ten. In many instances the seam which has been interrupted lies between two seams that have by extensive workings been proved to be entirely unaffected by such disturbances. I have on several occasions passed entirely across the site of a 'wash-out' in the workings of seams lying either above or below, thus demonstrating that the phenomena are confined to a single seam and the strata immediately adjacent to it; usually the seat-earth itself is unmoved.

It has been suggested that all the violent displacement and overridings are brought about by tectonic agency, and that they are thrust-planes. The localisation to a single stratigraphical plane should suffice to discredit this explanation, but it is still more definitely refuted by the fact that, in reply to questions put to every colliery manager I encounter, I have heard of only three examples of faults of the reversed or overlap type in the whole coalfield, two of which accomplished a displacement of only 3 or 4 feet. An amplification of the same explanation ascribes the displacements to a thrust with a movement from S.E. to N.W. and a common cause to the cleat or cleavage of the coal which is normally directed to the N.W. It suffices to refute this to remark that the wash-outs I have explored in this coalfield are aligned in four principal directions, so that if superposed they would give what may be called the Union Jack pattern, *i.e.* N.E.—S.W., N.W.—S.E., N.—S., and E.—W.

Moreover, if these so-called 'wash-outs' are not due to the erosive effects of contemporaneous or sub-contemporaneous streams, but to flat hading faults, any coal displaced should be presently found again without any loss whatever. That swellings and duplications of the seam occur we have already noticed, and such phenomena have been pointed to as evidence that there is 'no loss' of coal in connection with the so-called wash-outs. But losses and the gains by duplication do not, in fact, balance. A simple and convincing case is a wash-out in a thin seam of coal only 1 ft. 9 in. in thickness at Mirfield, in which, by taking measurements of the thickness of coal present and the breadth of the barren area, I have been able to show that a gap with no coal

for 210 feet is compensated for by only 35 feet of excess on the margin.

Seismic Phenomena in the Seams.

While the displacements and duplications are totally unlike those produced by faults, there are cases in which the seam appears to have been subjected to a stretching tension and to have broken under the strain. Along the zone of such a stretch great confusion is commonly found. Masses of sedimentary materials of the coal seam, and slabs and seams of cannel commonly occur, besides a curious argillaceous substance unlike any natural rock with which I am acquainted. In its unstratified structurelessness it suggests a kind of consolidated sludge such as might be produced by violently stirring or shaking a quantity of not too liquid mud. Where the seam abuts against this stuff it presents usually a nearly vertical ragged edge, its bright and dull layers preserving their characteristics quite up to the contact.

Masses of the seam enclose streaks of sandstone or muddy material along the bedding planes, and plates of sandstone descend in tortuous folds in the body of the seam. Sometimes 'eyes' of sandstone are seen embedded in the coal without any visible feeders, though in most cases the feeders, even almost hair streaks, can be discerned.

In many instances the sandstones in a wash-out of this type are found to be in great boat-bottom rolls, and even the whole sedimentary contents of the wash-out may lie in recumbent folds. The degree in which these disturbances are developed varies extremely; for example, at Shirebrook and Steetley Collieries there is no complication of any description either in the seam—the Top Hard or Barnsley Bed—or on the margins of the infilling of the 'wash-out.' At Manton, probably on the same wash-out, not more than two miles away, though there is only a small amount of swelling of the seam on the margin and a little injected sandstone in the coal, the filling of the wash-out was for some distance in perfectly horizontal recumbent folds of more than the full height of the workings. In this case it is interesting to observe that there were many tree-trunks represented by bright coal of great brilliance, and I observed one large Calamite standing in the position of growth in the undisturbed material of the filling.

I would illustrate by a concrete example—the great 'wash-out' represented by the Haigh Moor Rock is accompanied by a disturbance of the seam of portentous magnitude. In a range of four coterminous collieries the seam exhibits dislocations and over-riding repetitions and other anomalies along a general S.W.-N.E. line, coinciding roughly with the course of a normal steep hading fault of considerable magnitude. In many places the disturbance just along its edge culminates in over-ridings and repetitions whereby the thickness of the seam is increased from the normal 4 feet 6 inches up to 15 and 16 feet in some places, but this excess of coal is restricted to a narrow belt, while the default extends to scores or even to hundreds of yards.

That there is a connection between wash-outs and tectonic features I have long believed, and I pointed out some seventeen years ago[7] that

[7] *Quart. Jour. Geol. Soc.*, vol. lxi., p. 344.

the connection between great normal faults and the occurrence of wash-outs was too close to be merely fortuitous. But what the cause might be I was quite unable to suggest, and it was not until many years had elapsed that enlightenment came from a wholly unexpected quarter.

In brief, the explanation I have offered in a communication to the Geological Society of London, in a paper that has not yet been placed in full before that body, is that all these disturbances which complicate the already complex features of wash-outs are the effect of the lurching of the soft alluvial materials by earthquake agency. The present is not the occasion for amplifying the preliminary account of my evidence and argument published in the *Proceedings of the Geological Society* (No. 1,031, Jan. 17, 1919), but I may say this, that every predicable subterranean consequence of earthquake action upon unconsolidated alluvial deposits, such as the Coal Measures were, can be seen in the Yorkshire Coalfields. The lurchings, the rolling and heaving of sand-beds, the shaking to pulp of the muddy deposits, the rending and heaving of the peat, cracks in the peat, and cracks infilled with extraneous material passing through the strata; and lastly, though actually the first clue to the explanation, masses of sandstone in the form of inverted cones (' dog's-teeth,' ' paps,' or ' drops '), descending on to coal-seams, which I interpret as the deep-seated expression of the sand-blows that are the invariable accompaniments of earthquakes in alluvial tracts.

Let us imagine an earthquake sweeping across an alluvial plain beneath which lay a thick bed of water-charged peat overlain by laminated clay, and that in turn by sand and an upper layer of mud or clay, the impulse would throw the peat and its watery contents into a state of severe compression which would result in the bursting of the immediate cover of clay and the injection of water into the sand, and probably a large quantity of gas, converting it thus into quicksand. This in turn under the stress of the earthquake would eject water in the form of fountains through the upper muddy or silty stratum, producing sand-blows and craters on the surface. When the disturbance subsided sand would run back down the orifice into the funnel above the peat. These are the ' drops.' They are commonly flanged down the sides, showing that they were formed upon a line of crack. An earthquake not infrequently gives rise to permanent deformations of soft deposits either by the lurching of the surface and the production of permanent wrinkles, or by subterranean migration of quicksand so as to produce, here a sag or hollow, there a ridge or bombement. Mr. Myron Fuller's admirable account of the effects of the New Madrid earthquake of 1816 as observed one hundred years after the event is full of the most interesting and suggestive observations, not the least so those upon the sand-blows and sand-filled fissures containing lignite—the sand having come up from a bed lying at a depth of not less than 80 feet—the elevated tracts and the new lakes produced by subsidence. His photographic illustration of Reelfoot Lake with its broken and hollowed trunks of drowned trees must appeal to the imagination of every Coal Measure geologist.

Displacements or undulations of the surface of the Coal Swamps are readily traceable in many, perhaps in most, of the seams in this coalfield, but it is not always possible to prove their contemporaneity,

and especially is this the case with the rising folds. The depressions, however, are different. Our colliers apply the term 'swilly,' or sometimes 'swamp,' to shallow, trough-like inflections of a seam. These vary in depth from 2 or 3 feet up to as much as 50 or 60 feet; they vary greatly in breadth, but, so far as I have seen them, they are all steep-sided, perhaps 20° to 40°. Their linear extension ranges within wide limits; there is one at Rockingham which is known to extend for more than a mile. It has a breadth of 6 chains (132 yards) and a depth of 26 feet. A yet larger one traverses the whole extent of a colliery in Nottinghamshire. The evidence that these depressions were produced contemporaneously is in many cases decisive. Not only is the coal conspicuously thicker in a swilly than the normal, but the infilling is frequently of a different character from the normal roof material. In some cases it carries a patch of cannel; in others, while the normal roof passes over the swilly without bending, between coal and roof, a muddy deposit levels up the hollow Swillies are peculiar to a given seam, and I have learned of only one case in which more than one seam is affected by the same fold; but here it is also noted that, as in all wash-outs, the swillies are anterior to, and are thrown by, the faulting.

It seems probable that the isolated patches of cannel by which some coal-seams are surmounted may, in other cases than those of swillies, lie in hollows produced by earthquake deformation; and Fuller's picture of Reelfoot Lake tempts the reflection that upon its floor the maceration of peat into cannel substance may now be proceeding. If it were not so distant I would fain test it with a few probings.

The 'Cleat' or 'Slynes' of Coal.

One feature of coal-seams I must discuss before I conclude, though it will not at first appear clear that it can be brought within the title of this address—I allude to the cleavage or cleat or slynes of coal. If we look at a piece of coal this cleavage is very conspicuous, for, lying at right angles with the bedding, it gives the straight sides to the fragment. It is obviously not, like the cleavage of slate, *a texture*, but it is a series of well-developed joints varying in their individual vertical extension, some being restricted to a single layer of bright coal, and here and there one traversing bright and dull and fusain alike. A thick layer of fusain very commonly interrupts most of the cleat planes that have affected the other materials, and it is seen in such instances that chips of woody texture lie quite across the ineffective cleat.

It is a vital element in the cleat problem that it is as well developed and as definite in direction in a flake of bright coal the $\frac{1}{100}$th of an inch in thickness as in a tree-trunk. While I was preparing this address I procured a slab of shale from the bed underlying the uppermost bed of the Millstone Grit. It bore numerous imprints of goniatites and a leaf of Cordaites, which, in its present condition of bright coal, varies in thickness from about $\frac{1}{50}$th down to $\frac{1}{150}$th of an inch in thickness. It is traversed by an even and regular cleat at intervals of about $\frac{1}{100}$th of an inch, disposed at an angle of about 35° to the length of the leaf.

With great care it was possible to replace the slab in its original position and to determine the orientation of the cleat to be N.W.-S.E. This is not nearly the extreme of tenuity reached by well-cleated plant remains. I have specimens that are mere shiny films, and cannot, I should judge, exceed $\frac{1}{500}$th of an inch, yet they show well-defined and regular cleat. Further, it should be noted that the production of cleat was subsequent to the erosion of stream channels as well as to the production of phenomena on the margins of the wash-outs. Every pebble and flake of coal found in the displaced masses in these stream-casts has the cleat well developed, and in strict parallelism with the cleat of the adjacent undisturbed seam. Whether its production was later than the faulting has not been determined, and perhaps is indeterminable, as the faults have not been shown to *rotate* the strata; but, in the argument which follows, grounds will appear for regarding the cleat as produced before the induration of the strata, and the faulting has evidently happened in the main after consolidation.

In a paper which I contributed to the *Geological Magazine* in 1914, I directed attention to the fact that cleat is quite independent of the joints traversing the shales and sandstones of the associated measures; whence I drew the inference that the cleat must have been produced prior to the jointing, for had the two sets of divisional planes been produced simultaneously the agency that gave direction to the one would have influenced the other, while if the jointing had been produced first the coal could not possibly have escaped fracture by the joints. On the other hand, if the intimate and regular cleating preceded the production of the joints, no fresh fracturing would be requisite, or possible. But the jointing of the measures may, or rather, must, be regarded as an incident of the consolidation, so that, as a necessary corollary, we must regard the cleating of the coal as preceding the consolidation of the sediments in which the seams lay. This presents no *a priori* difficulty, and it is corroborated by experience of lignite in unconsolidated strata; for example, a bed of bright lustrous lignite lies interbedded in the wholly soft and incoherent Eocene sands and clays of Alum Bay in the Isle of Wight. This lignite, I find from a specimen collected before my interest was aroused, is divided by a definite cleat, but I did not make any observation of its direction.

The reason for this early development of a joint system is easily found—the original peat, in passing into lignite, acquired a brittle consistency and a consequent disposition to joint. Indeed, the change of consistency is the effect of chemical change and loss, whereby the peat substance contracts. Hence, when our Coal Measures were first laid down they would consist of a series of incoherent sands and muds, and this uncompacted condition may have persisted for a very long period so long as pressures were not excessive and no cementation took place; even surviving considerable tectonic disturbances, if we may judge by the conditions of the Bovey Tracey beds. The peats, however, would be subject to changes entirely innate: the gradual loss of volatile constituents, or at least the resolution of the carbon compounds into new groupings and the conversion of the mother substance of the coal into lignite. In this condition the coal-substance would be brittle and liable

to joint in response to the tensile strains set up by the contractility of the mass.[8]

There are questions of very deep import concerned with the geographical direction of the cleat. The first reference to this interesting topic is, I believe, in a work, close upon a century old, by Edward Mammatt, entitled *Geological Facts to elucidate the Ashby-de-la-Zouch Coalfield*, published in 1834. His fourth chapter, headed 'On the polarity of the strata and the general law of their arrangement,' contains these remarkable passages: 'Polarity of the strata is a subject which hitherto has not been much considered. The extraordinary uniformity in the direction of the slynes and of the partings of the rocky strata seems to have been determined by the operation of some law not yet understood. . . . Wherever these slynes appear, their direction is 23° West of North by the compass, whatever way the stratum may incline. The coal between them has an arrangement of lines all parallel to the slynes, by which it may be divided. This is called the *end* of the coal. . . . Many of the Derbyshire and Nottinghamshire Coal Measures have their slynes in the same arrangement as the strata upon Ashton Woulds, and this is also preserved in those of Coleorton, about five miles to the westward.'

In my paper in the *Geological Magazine* I commented on the fact that little had been written on the subject of cleat since Jukes' *Manual of Geology* (1862), in which he quotes a Nottinghamshire miner's remark that the slyne faced 'two o'clock sun, like as it does all over the world, as ever I heered on,' a generalisation to be remembered.

John Phillips, in a report presented to this Section in the year 1856, corroborates the statement so far as concerns the coalfields of Northumberland and Durham, where he says it 'runs most generally to the north-west (true).' The same direction, he says, prevails in Yorkshire and Derbyshire and also in Lancashire.

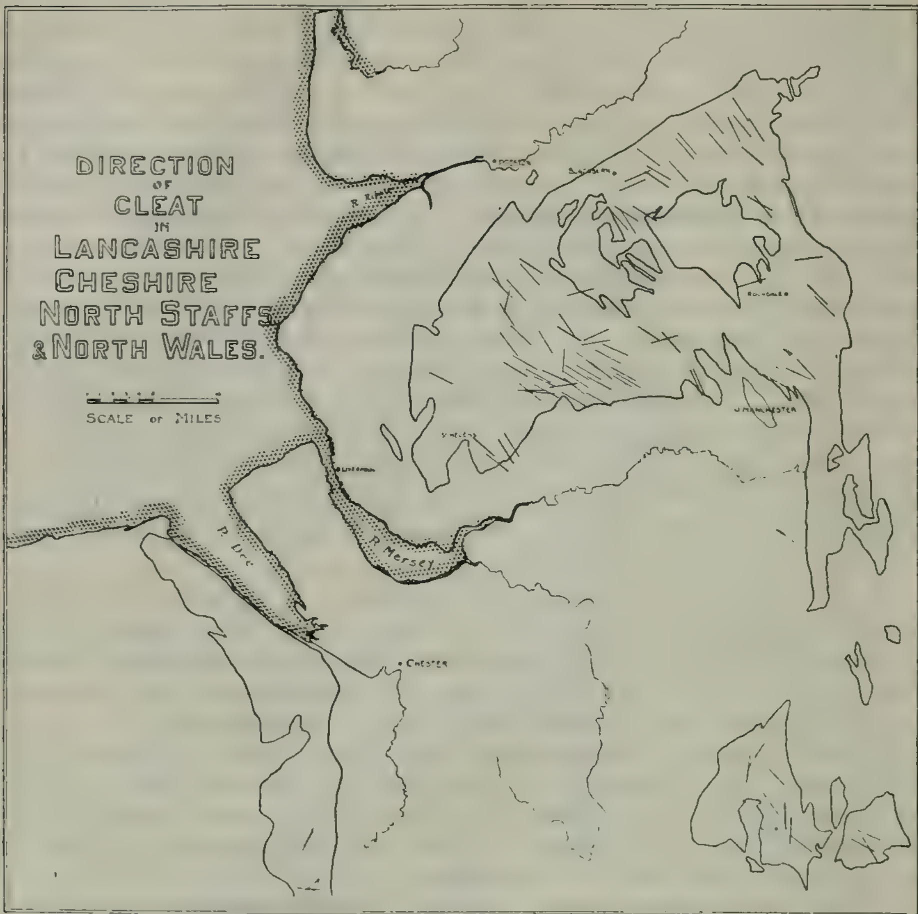
I have suggested a reason why coal should acquire a joint system anterior to, and independent of, that of the associated measures, but while providing a jointing-force that theory furnishes no explanation of the directional tendency of the cleat. This tendency must have been supplied by some directive strain—not necessarily of great intensity, but continuous in its operation.

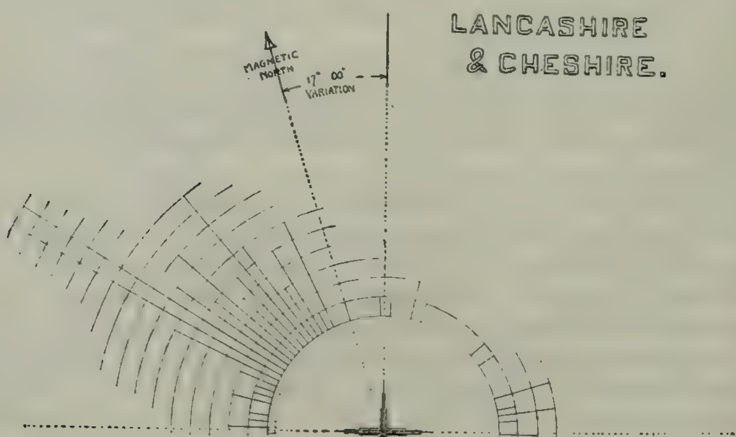
The idea that the initiation of joints, as it were the pulling of a trigger, is due to seismic tremors, is urged by Mr. W. O. Crosby, but it seems that an agency much more constant in operation and direction is required.

In 1914 and since I have collected a great body of data regarding the direction of the cleat in coals and lignites in many parts of the world, chiefly by means of circular-letters to every colliery manager in the British Isles and to many abroad. I have also obtained most generous help and information from valued correspondents in the United States, foremost of whom I must mention Professor J. J. Stevenson.

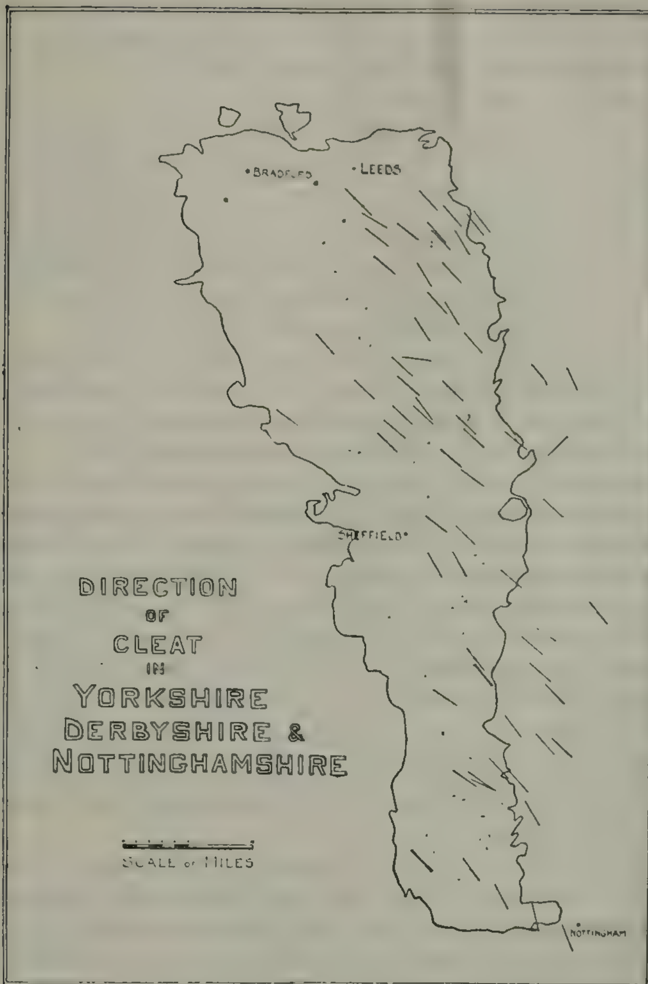
Cleat observations in the Northern Hemisphere show an overwhelming preponderance of a N.W.-S.E. direction in coals and lignites of all

[8] Fusain, being already greatly decomposed, would not be as brittle and would not cleat so readily.





LANCASHIRE & CHESHIRE.


LEEDS
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DIRECTION
OF
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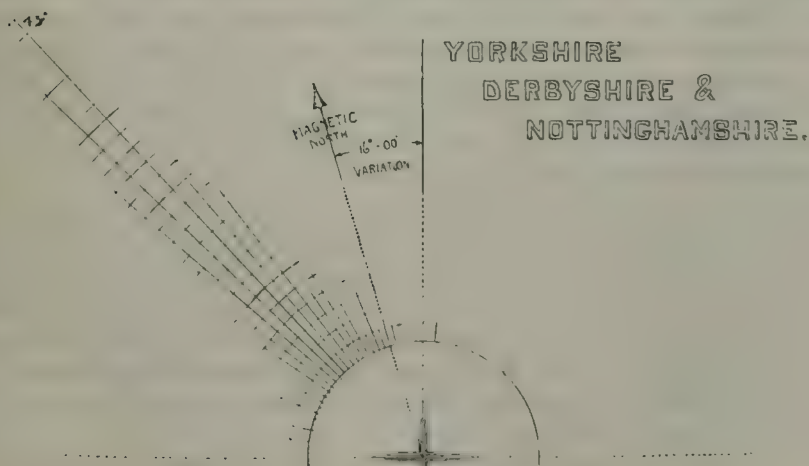
MAGNETIC NORTH
16°·00'
VARIATION
YORKSHIRE
DERBYSHIRE &
NOTTINGHAMSHIRE.

ages from Carboniferous to Pleistocene and from regions as remote as Alaska, Spitsbergen, the Oxus, Nigeria, and China. This direction persists through every variety of tectonic relations, but seems most regular in the largest and least disturbed fields.

Jukes' miner's astonishing statement that 'the slyne faces two o'clock sun . . . all over the world' involves more than is at first glance apparent, for, as a friend has pointed out—and when one gives the matter a thought it is obvious—that two o'clock sun must shine from a quite different compass-bearing in the Northern and Southern Hemispheres. Yet the data I have collected confirm generally the miner's declaration in the Southern Hemisphere as well as the North, though exceptions occur that may possess a deep significance.

Many of the southern coals have no definite cleat, but in such as do display a regular system there is a distinct predominance of the N.E.-S.W. direction, which has a curious inverse relationship with the N.W.-S.E. direction of the Northern Hemisphere.

With war-time interruptions of my inquiries, and, since the war, a spirit of unrest in the mining world which is not conducive to scientific research, I do not feel that the matter is ripe for full discussion, and I forbear to disclose the speculations as to cause which are confided to my note-books further than to say that I feel persuaded that the cause will be found in some relation to influences, tidal or other, dependent upon the earth's planetary *rôle*. I have reason to believe that some of the information sent to me from distant fields went to the bottom of the sea in the submarine war. When such deficiencies are made good, and all the data gathered together, will be time enough to invoke the aid of specialists in the department of Mathematics most concerned with questions of this nature.

Meanwhile I would invite attention to the case of another type of organically formed rock that shares with coal the capacity for early consolidation—namely, limestone. My attention was long ago attracted by a remarkable bed of limestone in the gorge at Gordale. It is, at a guess, 100 feet in thickness, and is distinguished by a remarkable system of vertical joints that split the mass into thin plates ranging from half an inch up to several inches in thickness. Determinations of the direction of jointing are not easily made, as the plates are irregular, but a series of eleven determinations made for me gave a maximum deviation of $11\frac{1}{2}$ degrees on each side of the mean value N.41°W. (true bearing), which agrees remarkably with the jointing of the more normal limestones in the district and also with the jointing of the chalk over large areas of the south-east of England.

There is a negative aspect of the cleat question which brings it more clearly within the ambit of an inquiry into the physiography of the coal swamps. I allude to the absence of cleat that characterises anthracite the world over, and is the basis for the broad classification of coals in the United States into cubical and non-cubical coals. Upon this absence of cleat is attendant features that have been regarded as indicative of conditions prevailing during the formation of the coal, and hence clearly within my terms of reference.

In the Memoir of the Geological Survey on the Coals of South Wales

it is pointed out that the anthracite condition, instead of being accompanied by a high ash-content—which is what might be expected if the ash ratio were determined simply by the reduction in the non-ash—is shown statistically to bear the reverse relationship. That is, the more anthracitic the coal, the lower the ash. From this it is argued that the anthracites of South Wales were formed of plant-constituents different from those contributing to the steam and house coals. This proposition gains no support from the study of the plants found in the associated measures, nor does it explain why the coals of other fields, composed in their various parts of very diverse constituents, do not exhibit the anthracite phase. But the ash question needs to be approached from another point of view. The ash of coal may, as I have shown elsewhere, be composed of three entirely distinct and chemically different materials. There may be (1) the mineral substances belonging to the plant-tissues; then (2) any detrital mineral substances washed or blown into the area of growing peat; and, finally, the sparry minerals located in the lumen of the cleat. As to the first, I have long considered that the coal was in large measure deprived by leaching of much of its mineral substances; it is otherwise difficult to account for the almost total absence of potash. The second—detrital matter—is probably present in some though not in all coals; the high percentage of aluminium silicate is probably of this origin. But the third constituent—the sparry matter—may, both on *a priori* grounds and upon direct evidence, be assigned a very important *rôle* in the production of the ashes in most coals.

When a coal with a strongly developed cleat is examined in large masses it is at once seen that the cleat spaces are of quite sensible width, and that they are occupied most commonly by a white crystalline deposit which may consist of either carbonate of iron or carbonate of lime, and there are also in many seams crystals of iron sulphide—either pyrites or marcasite. These sparry veins may be as much as $\frac{1}{10}$th of an inch, or even more, in thickness, and they clearly constitute the principal contributors to the ash. It has been suggested that they are true components of the original peat, a proposition to which no botanist would assent, and it appears certain that the veins consist of material introduced by percolation from the overlying measures, subsequent to the production of the cleat. If that be so, it then will follow that the amount of the material present in coal must be in some direct proportion to the available cleat space, and if there is no cleat neither will there be any vein-stuff to contribute to the ash. It should be pointed out that ordinary bituminous coal broken into minute dice and washed so as to remove any heavy mineral particles is found to contain a percentage of ash quite comparable with that of an average anthracite. It is to be concluded, therefore, that the variations of the ash contents of a coal are no indication of the plant-constituent of the coal.

I have sought to show how the concept of the Coal Measures with their sandstones, shales and coal-seams accords entirely with what we know of modern swamps and deltas, and that just as each Coal Measure fact finds its illustration in modern conditions, so we may, inverting

the method of inquiry, say that no noteworthy features of the modern swamps fail to find their exemplification in the ancient.

Even what may seem the most daring of my propositions—the seismic origin of abnormal 'wash-outs'—finds, I cannot doubt, a full justification in what has been *seen* in the Sylhet region by Mr. Oldham, and in the Mississippi valley by Mr. Fuller, or in what can be *inferred* as a necessary subterranean accompaniment of these surface signs of great earthquake convulsions.

One, and one only, Coal Measure phenomenon lacks its obvious modern parallel, the cleat, and hereon I present the complement to the text of this address—the ton of fact awaiting the illuminating ounce of theory that shall outvalue it.

THE PROGRESSION OF LIFE IN THE SEA.

ADDRESS TO SECTION D (ZOOLOGY) BY

E. J. ALLEN, D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

The method we usually follow in the ordinary course of zoological work is to make first, with the unaided eye, a general examination of the animal that interests us, and then study in detail its separate parts with a simple lens, with a low power of the microscope, with gradually increasing powers, until, finally, minute portions are examined with the highest oil-immersion lens. The successful research worker is generally one who, whilst carrying to the utmost limit he can achieve his search into detail, maintains as by instinct a true sense of proportion and holds firmly to the idea of the organism as a whole.

In discussing the living organisms of the sea I shall try to follow a similar plan, thinking of the life of the sea as a whole, built up of individual plants and animals, each in intimate relation with its surroundings, and all interdependent among themselves. But even this is not enough, for we must take still a wider view and keep in mind not only the life of the waters, but that also of the land and of the air, for both, as we shall see, have a bearing on our theme. Deep oceans, coastal waters, shallow seas, rivers and lakes, continents and islands, all play their part in one scheme of organic life—life which had, it seems, one origin, and notwithstanding migrations and transmigrations from water to land, from land to air, and from land and air back again to the water, remains one closely inter-related whole.

Both Brandt[1] and Gran[2] have recently emphasised the fact that it is in the coastal waters and shallow seas, which receive much drainage from the land, that plant and animal life are most abundant, the more open oceans far from land being relatively barren; as Schütt puts it, the pure blue of the oceans is the desert colour of the seas. This increased production in the coastal waters is due principally to the presence of nitrogen compounds and compounds of phosphorus derived from terrestrial life. From forest, moor and fen, wherever water trickles, the life of the land sends its infinitesimal quota of these essential foodstuffs to fertilise the sea.

When, however, we go back to the beginning of things, we shall probably be right if we say that any influence of terrestrial life upon life in the sea must be left out of account. Different views are still

[1] *Wissensch. Meeresunters.* Kiel, 18, 1916-20, p. 187.
[2] *Bull. Planktonique.* Cons. Internat., 1912 (1915).

held as to where life in the world had its origin, but no one questions that it began in close connection with water. That it began in the sea, where the necessary elements were present in appropriate concentrations and in an ionised state, is an idea which appeals to many with increasing force the more closely it is examined. This view has been developed recently by Church[3] in his memoir on 'The Building of an Autotrophic Flagellate,' in which he boldly attempts to trace the progression from the inorganic elements present in sea-water to the unicellular flagellate in the plankton phase, floating freely in the water. The autotrophic flagellate, manufacturing its own food, he regards as the starting-point from which all other organisms, both plants and animals, have sprung. To understand the first step in this progression, the passage from the dead inorganic to the living organic remains, as it has always been, one of the great goals of science, not of biological science alone, but of all science. Recent research has, I think, thrown much light on the fundamental problems involved. In a paper published last year, Baly, Heilbron, and Barker,[4] extending and correcting previous work by Benjamin Moore and Webster,[5] have shown that light of very short wave-length ($\lambda = 200\ \mu\mu$), obtained from a mercury-vapour lamp, acting upon water and carbon dioxide alone, is capable of producing formaldehyde, with liberation of free oxygen. Light of a somewhat longer wave-length ($\lambda = 290\ \mu\mu$) causes the molecules of formaldehyde to unite or polymerise to form simple sugars, six molecules of formaldehyde, for example, uniting to form hexose. The arresting fact brought out in these researches is that the reactions take place, under the influence of light of appropriate wave-lengths, without the help of any catalyst, either organic or inorganic. Where a source of light is used which furnishes rays of many wave-lengths, the simple reaction of the formation of formaldehyde is masked by the immediate condensation of the formaldehyde to sugar, but this formation of sugar can be prevented by adding to the solution a substance which absorbs the longer wave-lengths, so that only the short ones which produce formaldehyde are able to act.

When the formation of sugars is postulated, the introduction of nitrogen into the organic molecule offers little theoretical difficulty; for not only has Moore[6] shown that nitrates are converted into the more chemically active nitrites under the influence of light of short wave-length, but he maintains that marine algæ, as well as other green plants, can under the same influence assimilate free nitrogen from the air. Baly[7] also has succeeded in bringing about the union of nitrites with active formaldehyde in ordinary test-tubes by subjecting the mixture to the light of a quartz-mercury lamp.

[3] *Biological Memoirs*, I. Oxford, 1919.

[4] *Journ. Chem. Soc.*, London, vols. 119 and 120, 1921, p. 1025. *Nature*, vol. 109, 1922, p. 344.

[5] *Proc. Roy. Soc.* B., vol. 87, p. 163 (1913), p. 556 (1914); vol. 90, p. 168 (1918).

[6] *Proc. Roy. Soc.* B., vol. 90, p. 158 (1918); vol. 92, p. 51 (1921).

[7] Baly, Heilbron and Hudson, *Journ. Chem. Soc.* London, vols. 121 and 122, 1922, p. 1078.

It will be admitted that these three reactions: (1) the formation of formaldehyde, H.CO.H, from carbonic acid, OH.CO.OH, with liberation of free oxygen, or, to put it more simply, the direct union of the carbon atom of CO_2 with a hydrogen atom of H_2O; (2) the formation of sugars from formaldehyde, and (3) the formation from nitrites and formaldehyde of nitrogenous organic substances, are the most fundamental and characteristic reactions of organic life. It is true that light of such short wave-lengths ($\lambda = 200\,\mu\mu$) as were required in Baly's experiments to synthesise formaldehyde do not occur in sunlight as it reaches the earth to-day; but, as we shall see later, the same author has shown that, in the presence of certain substances known as photocatalysts, the reaction can be brought about by ordinary visible light; and from Moore and Webster's work it appears that colloidal hydroxides of uranium and of iron are suitable photocatalysts for the purpose.

If these results of the pure chemist are justified, they go far towards bridging the gap which has separated the inorganic from the organic, and make it not too presumptuous to hazard the old guess that even to-day it is possible that organic matter may be produced in the sea and other natural waters without the intervention of living organisms. We may note here, too, that if we take account of only the most accurate and adequately careful work, the actual experimental evidence at the present time requires the presence of a certain amount of organic matter in the culture medium or environment before the healthy growth of even the simplest vegetable organism can take place. This was illustrated in some experiments made by myself some years ago when attempting to grow a marine diatom, *Thalassiosira gravida*, in artificial sea-water made up from the purest chemicals obtainable dissolved in twice-distilled water. Even after nutritive salts, in the form of nitrates and phosphates, had been added, little or no growth of the diatom occurred. But if as little as 1 per cent. of natural sea-water were added excellent cultures resulted, in which the growth was as healthy and vigorous as I was able to obtain when natural sea-water was used entirely as the basis of the culture medium. There was clearly some substance essential to healthy growth contained in the 1 per cent. of natural sea-water, and from further experiments it became practically certain that it was an organic substance. When, for instance, the natural sea-water was evaporated to dryness, the residue slightly heated and redissolved in distilled water, 1 per cent. of this solution added to the artificial culture medium was as potent in producing growth of the diatom as the original natural sea-water had been. When, on the other hand, the residue after evaporation was well roasted at a dull red heat and redissolved in distilled water, the addition of this solution to the artificial culture medium produced no effect and growth did not take place. Growth could also be stimulated by boiling a small fragment of green seaweed (*Ulva*) in the artificial culture medium, the weed being removed before inoculation with the diatom. All this points to the necessity for the presence of some kind of organic matter in the solution before growth can take place. One must not dogmatise, however, for there are many pitfalls in the experimental work and the necessary degree of accuracy is difficult to attain. My own experience

of these difficulties culminated when I discovered, covering the bottom of my stock bottle of distilled water—water which had been carefully redistilled from bichromate of potash and sulphuric acid in all-glass apparatus—a healthy growth of mould.

Let us then assume that we are allowed to postulate in primitive sea-water or other natural water organic compounds formed by the energy of light vibrations from ions present in the water, and see how we may proceed to picture the building up of elementary organisms. Without doubt the evolutionary step is a long and elaborate one, for even the simplest living organism is already highly complex both in structure and function. As the molecules grew more complex by the progressive linkage of the carbon atoms of newly formed carbohydrate and nitrogenous groups, we must suppose that the organic substance, for purely physical reasons, assumed the colloidal state, and at the same time its surface-tension became somewhat different from that of the surrounding water. With the assumption of the colloidal state, the electric charges on the colloidal particles would produce the effect of adsorption and fresh ions would be attracted from the surrounding medium, producing a kind of growth entirely physical in character. We thus arrive at the conception of a mass of colloidal plasma differing in surface-tension from the water and increasing in size by two processes, the one chemical, due to linkage of carbon atoms; the other physical, brought about by the adsorption of ions by the colloidal particles.

The difference of surface-tension would tend to make the surface a minimum and the shape of the mass spherical. On the other hand, maximum growth would demand maximum exchange with the surrounding medium, and hence maximum surface. From the antagonism of these two factors, surface-tension and growth, there would follow, firstly, the breaking up of the mass into minute particles upon the slightest agitation, and, secondly, changes of form wherever growth involved local alterations of surface-tension, which changes of form would represent the first indication of the property of contractility.

So far we have considered only the process of the building up of the elementary plasmic particles, the anabolic process. Church, whose memoir already referred to I am now closely following, points out that these anabolic operations must from the beginning have been subject to the alternations of day and night, for during the night the supply of external energy is removed. 'If during the night,' he asks, 'the machine runs down, to what extent may it be possible so to delay the onset of molecular finality that some reaction may continue, at a lower rate, until the succeeding day?' And his answer is: 'The successful solution of this problem is defined physiologically by the introduction of the conception "*katabolism*," as implying that energy derived from the "breaking down" of the plasma itself . . . may be regarded as a "secondary engine," functional in the absence of light, and evolved as a last resort in failing plasma.' Katabolism persists as the ultimate mechanism in the physiology of animal as contrasted with plant life, but if the suggestion just quoted is sound it originated, as the first 'adaptation' of the organism, to meet the factor of recurring night

and day. That the problem was successfully solved we know, but as to the mechanism of its solution we have no key. It is at this point again, to use Church's words, that the 'plasma, previously within the connotation of chemical proteid matter, becomes an autotrophic, increasingly self-regulated, and so far individualised entity, to which the term "life" is applied.'

The elementary plasma is thus now fairly launched as an individual living organism, and the great fundamental problems of biology—memory, heredity, variation, adaptation—face us at each step of our further progress. We see in broad outline the conditions the advancing organism had to meet, we see the means by which those conditions were in fact met, we know that only those individuals survived which were able to meet them. Further than this we, the biologists of to-day, have not advanced. The younger generation will pursue the quest, and, with patient effort, much that now lies hidden will grow clear.

The differentiation of the growing particles of plasma into definite layers, which followed, seems natural; first the external layer, in molecular contact with the surrounding water, from which it receives substances from outside in the form of ions, and to which it itself gives off ions; beneath this the autotrophic layer to which light penetrates, and in which, under the influence of the light, new organic substance is built up; in the centre a layer to which light no longer penetrates. This central region, the nucleus, depends entirely on the peripheral layers for its own nutrition, and becomes itself concerned only with katabolic processes, those processes of the organism which depend upon the breaking down, and not the building up, of organic substance.

At an early stage in the development of the individual organism the spherical shape, which the organic plasma was compelled to assume under the influence of surface-tension, underwent an important modification, the effect of which has impressed itself upon all later developments. A spherical organism floating in the water and growing under the direct influence of light would obviously grow more rapidly on the upper side, where the light first strikes it, than it would on the lower side away from the light. There followed, therefore, an elongation of the sphere in the vertical direction, and the definite establishment of an anterior end, the upper end which lay towards the light and at which the most vigorous growth took place. In this way there was established a definite polarity, which has persisted in all higher organisms, a distinction between an anterior and a posterior end. With the concentration of organic substance which took the form of nucleus and reserve food supply, the specific gravity of the plasma would become greater than that of the surrounding water and the organism would tend to sink. The necessity, therefore, arose for some means of keeping it near the surface, that it might continue to grow under the influence of light. The response to this need, however it was attained, came in the development of an anterior flagellum. This we may regard as an elongation in the direction of the light of a contractile portion of the external layer, moving rhythmically, which by its movement counteracted the action of gravity, and acting as a tractor drew the primitive flagellate upwards towards the surface layers, into a position where further growth

was possible. That this speculation of Church's represents what was actually accomplished, even though it does not make clear the means by which it was brought about, is shown by the interesting researches of Wager [8] on the rise and fall of the more highly organised flagellate *Euglena*. *Euglena* is a somewhat pear-shaped flagellate, the tapering end being anterior and provided with a single flagellum, which acts as a tractor drawing the organism towards the light. The posterior end carries the nucleus and most of the chlorophyll and food reserves. The whole organism has a specific gravity of 1.016, being slightly heavier than the fresh water in which it lives. When dead, or when the flagellum is not moving, it takes up, under the action of gravity alone, a vertical position in the water, with the pointed anterior end uppermost, and the heavier, rounded, posterior end below, and sinks gradually to the bottom.

In a very crowded culture a curious phenomenon is seen, because the organisms tend to aggregate into clusters beneath the surface film, and when they are crowded together in these clusters the flagella cease to work. This makes the whole cluster sink to the bottom under the action of gravity. When the bottom is reached the individuals are spread out by the action of the downward current, and, when they are sufficiently widely apart, the flagella again begin to move, carrying the organisms in a more diffuse stream once more to the surface. The whole culture vessel becomes filled with a series of vertical lines of closely aggregated falling organisms, surrounded by a broad cylinder of disseminated swimming ones, rising to the surface by the action of their flagella. If the conditions are kept uniform such a circulation of *Euglenas*, falling to the bottom by gravity when the flagella are stopped and returning to the surface under their own power, will continue for days.

The flagellum in this species, therefore, retains its most primitive function of drawing the organism to the light in the surface layer. With the establishment of the flagellum an organ is produced which shows remarkable persistence in both the animal and vegetable kingdoms, and from the existence of the flagellated spermatozoon in the higher vertebrates, in accordance with Haeckel's biogenetic law that the individual in its development repeats or recapitulates the history of the race, we conclude that they also in their earliest history passed through a plankton flagellate phase.

Exactly at what stage in the history of the autotrophic flagellate the first formation of chlorophyll and its allied pigments took place we have no means of determining, but it may have been before even the flagellum itself had begun. This advance and the subsequent concentration of the pigments into definite chromatophores or chloroplasts doubtless immensely increased the efficiency of the organism in producing the food which was necessary to it. The recent work of Baly and his collaborators becomes here again of the first importance, and though the subject of the part played by chlorophyll in photosynthesis

[8] *Phil. Trans. Roy. Soc.*, vol. 201, 1911; and *Science Progress*, vol. vi., October 1911, p. 298.

belongs rather to botany and chemistry than to zoology, I may perhaps for the sake of completeness be allowed to refer to it very briefly. I have already said that Baly brought about the synthesis of formaldehyde from CO_2 and H_2O under the influence of rays of very short wave-length ($\lambda = 200\mu\mu$) from a mercury-vapour lamp. He was also able to show that when certain coloured substances were added to the solution of carbon dioxide in water the same reaction took place under the influence of ordinary visible light. His explanation of this process is that the coloured substance known as the photocatalyst absorbs the light rays and then itself radiates, at a lower infra-red frequency corresponding to its own molecular frequency, the energy it has absorbed. At this lower frequency the energy thus radiated is able to activate the carbonic acid, so that the reaction leading to the formation of formaldehyde can and does take place. In the living plant this synthesised formaldehyde probably at once polymerises to form sugars.

Malachite green and methyl orange, as well as other organic compounds, were found to act as photocatalysts capable of synthesising formaldehyde, and Moore and Webster's work had previously shown that inorganic substances, such as colloidal uranium oxide and colloidal ferric oxide, can do the same. Chlorophyll in living plants may with some confidence be assumed to operate in a similar way, though no doubt the series of events is more complex, since the green pigment itself is not a single pigment, and others, such as carotin and xanthophyll, are also concerned.

We have tried to picture the gradual building up from elements occurring in sea-water of a chlorophyll-bearing flagellate, capable of manufacturing its own nourishment and able to multiply indefinitely by the simple process of dividing in two. If we assume only one division during each night as a result of the day's work in accumulating food material, such an organism would be able in a comparatively short space of time to occupy all the natural waters of the world. But here we are met by a difficulty which is not easily overcome. Chlorophyll, the photocatalyst, the most essential factor in the building up of the new organic matter, is itself a highly complex organic substance, and in any satisfactory theory its original formation and its constant increase in quantity must be accounted for. Lankester [9] has maintained that chlorophyll must have originated at a somewhat late stage in the development of organic life, and has suggested that earlier organisms may have nourished themselves like animals on organic matter already existing in a non-living state. An alternative hypothesis, which in view of the recent work seems more attractive, is to suppose that the earlier organisms were either activated by some simpler photocatalyst, or that they received the necessary energy at suitable frequency directly from some outside source.

It must not be forgotten, also, that at the time these developments were taking place the conditions of the environment would in many ways have been different from those now existing in the sea. One

[9] *Treatise on Zoology*, Part I, Introduction. London, 1909.

suggestion of special interest that has been made[10] is that the concentration of carbon dioxide in the atmosphere, and hence also in natural waters, was very much greater than it is to-day. Free oxygen, indeed, may have been entirely absent, and all the free oxygen now present in the air may owe its existence to the subsequent splitting up of carbon dioxide by the action of plant life. With such possibilities of differences in the conditions in this and in so many other directions, may we not be well satisfied if, for the time, we can say that the formation of carbohydrates and proteids has been brought within the category of ordinary chemical operations, which can occur without the previous existence of living substance?

To return once more, however, to the free-swimming, autotrophic flagellate. In the early stages of its history the loss caused by sinking, and so getting below the influence of light and the possibility of further growth, must have been enormous. We may conceive a constant rain of dead and dying organisms falling into the darker regions of the sea, and thus a new field would be offered for the development of any slight advantages which particular individuals might possess. Under such conditions we may suppose that the holozoic or animal mode of nutrition first began in the absorption of one individual by another, with which it had chanced to come into contact. If the one individual were more vigorous and the other moribund we should designate the process 'feeding,' and the additional energy obtained from the food might well cause the individual to survive. If the two individuals which coalesced were both sinking from loss of vigour, the combined energy of the two might make possible a return to the upper water layers, where under the influence of light growth and multiplication would proceed, and we should, I suppose, designate the coalescence 'conjugation,' or sexual fusion.

Other individuals, again, sinking in shallow water, would stick to solid objects on the sea-floor, whilst the flagellum continued to vibrate. The current produced by the flagellum under these conditions would draw towards the organism dead and disintegrating remains of its fellows, and again we should have ingestion and animal nutrition. At this stage we witness the definite passage from plant to animal life. A further stage is seen when a cup-like depression to receive the incoming particles of food is formed at the base of the flagellum, to be followed still later by a definite mouth.

Any roughening of the external surface of the swimming flagellate, such as we so often find brought about by the deposition of calcareous plates or siliceous spicules, or the production of ridges or furrows, would tend to slow down its speed of travel, from increased friction with the surrounding water. This would have a similar effect to actual fixation in drawing floating particles by the action of the flagellum, and also lead to animal nutrition. Still another development would occur when the fallen flagellate began to creep along the sea-floor by contractile movements of the plasmic surface, losing its flagellum, and adopting

[10] See Carl Snyder, 'Life without Oxygen,' *Science Progress*, vol. vi., 1912, p. 107.

the mode of life of an amœba. That amœba and its allies, the Rhizopods, are descended from a flagellate ancestor is a view suggested by Lankester[11] in 1909, which was adopted by Doflein,[12] and is now strongly advocated by Pascher[13] as a result of much new research.

The transformation from the plant to the animal mode of feeding we can see in action by studying actual organisms which exist to-day. In the course of my work already referred to on the culture of plankton organisms there has on several occasions flourished in the flasks a small flagellate belonging to the group of Chrysomonads, which was first described by Wysotzky under the name of *Pedinella hexacostata*, and to which I drew the attention of Section D at the Cardiff Meeting in 1920. The general form of Pedinella resembles that of the common Vorticella, but its size is much smaller. The body, which is only about 5μ in diameter, is shaped like the bowl of a wine-glass, and from the base of the bówl, which is the posterior end, a short, stiff stalk extends. From the centre of the anterior surface there arises a single long flagellum, surrounded at a little distance by a circle of short, stiff, protoplasmic hairs. Arranged in an equatorial ring just inside the body are six or eight brownish-green chromatophores or chloroplasts. In a healthy culture Pedinella swims about freely by means of a spiral movement of the flagellum, which functions as a tractor, the stalk trailing behind. The chromatophores are large, brightly coloured and well developed, and the organism is obviously nourishing itself after the manner of a plant, like any other Chrysomonad. But from time to time a Pedinella will suddenly fix itself by the point of the trailing stalk. The immediate effect of this fixing is that a current of water, produced by the still vibrating flagellum, streams towards the anterior surface of the body, and small particles in the water, such as bacteria, become caught up on the anterior surface, the ring of fine stiff hairs surrounding the base of the flagellum being doubtless of great assistance in the capture of this food. One can clearly see bacteria and small fragments of similar size engulfed by the protoplasm of the anterior face of the Pedinella and taken into the body. The organism is now feeding as an animal. In some of the cultures in which bacteria were very plentiful nearly all the Pedinella remained fixed and fed in the animal way, and when this was so the chromatophores had almost disappeared, though they could still be seen as minute dark dots. We can as it were in this one organism see the transition from plant to animal brought about by the simple process of the freely swimming form becoming fixed.

In the group of Dinoflagellates also—the group to which the naked and armoured peridinians belong—the same transition from plant to animal nutrition can be well followed by studying different members of the group. In heavily armoured forms, with a rich supply of chromatophores, nutrition is chiefly plant-like or holophytic. In those with fewer chromatophores there is, on the other hand, often distinct evidence of the ingestion of other organisms, and nutrition becomes

[11] Lankester, *Treatise on Zoology*, Part I., London, 1909, p. xxii.
[12] Doflein, *Protozoenkunde*, 1916.
[13] Pascher, *Archiv f. Protistenkunde*, Bd. 36, 1916, p. 81, and Bd. 38, 1917, p. 1.

partly animal-like. Amongst the naked Dinoflagellates such holozoic nutrition is very much developed, and in many species has entirely superseded the earlier method of carbonic acid assimilation.

It is really surprising how many structural features found in higher groups of animals make their first appearance in these naked Dinoflagellates in conjunction with this change of nutrition, and we seem to be led directly to the metazoa, especially to the Cœlenterata. In *Polykrikos* there are well-developed stinging cells or nematocysts, as elaborately formed as those of Hydra or the anemones. In *Pouchetia* and *Erythropsis* well-developed ocelli are found, consisting of a refractive, hyaline, sometimes spherical lens, surrounded by an inner core of red pigment and an outer layer of black; the whole structure is comparable to the ocelli around the bell of a medusa. In *Noctiluca* and in the allied genus *Pavillardia* a mobile tentacle, which is doubtless used for the capture of food, is developed. Division of the nucleus, with the formation of large, distinct chromosomes, has also been described in several of these Dinoflagellates. With the tendency of the cells in certain species to hold together after division and form definite chains we seem to approach still nearer to the metazoa, until, finally, in *Polykrikos* we reach an organism which may well have given rise to a simple, pelagic cœlenterate. It is difficult to resist the suggestion put forward by Kofoid[14] in his recent monograph, that if to *Polykrikos*, with its continuous longitudinal groove which serves it as a mouth, its multicellular and multinucleate body and its nematocysts, we could add the tentacle of *Noctiluca*, and perhaps also the ocellus of *Erythropsis*, 'we should have an organism whose structure would appear prophetic of the Cœlenterata and one whose affinities to that phylum and to the Dinoflagellata would be patent.' Or it may be that the older view is the correct one here, and that the first cœlenterate came from a spherical colony of simple holozoic flagellates, arranged something on the plan of *Volvox*, in which the posterior cells of the swimming colony, in whose wake food particles would collect, had become more specialised for nutrition than the rest.

Before proceeding, however, to consider the further progress of animal life, we must pause for a moment to ask in what direction plant life in the sea developed, from which the increasing animal life derived its nourishment. Here the striking fact is the lack of progress in the free, floating, plankton phase. The plant life of the plankton has never proceeded beyond the unicellular stage, for the plankton diatoms, which with the peridinians form the great, fundamental vegetable food supply of the sea, are only autotrophic flagellates which have lost their flagella, having acquired other means of flotation to keep them in the sunlit region of the upper water layers. Deriving their food, as these plants do, directly from molecules in the sea-water, the factor which is for them of supreme importance is the exposure of maximum surface directly to the water. Hence the minute unicellular form has been the only one to survive as phytoplankton. The marine region in which

[14] Kofoid and Swezy, 'The Free-living Unarmoured Dinoflagellata.' *Mem. Univ. California*, 1921.

plant life has succeeded in making some progress is the narrow belt along the shores, where a fixed life is possible, but this belt, limited by the amount of light which penetrates, extends only to a depth of about 15 fathoms. The available area is further restricted to rocky and hard bottoms, and is therefore nowhere great. This is the wave-lashed region of the brown and red sea-weeds. In the brown sea-weeds a history can still be traced,[15] from the fixture of an autotrophic flagellate to the building up, by laying cell on cell, of the essential structures which afterwards, on transmigration to the land, reached their climax in the forest tree.

But if the flagellate thus rose and gave origin to the flora of the land, it also degenerated, for it adopted a parasitic habit, living in and directly absorbing already formed organic matter. In this way the bacteria arose, whose activities in so many directions influence the life of to-day. This view exceeds in probability, I think, the suggestion often put forward,[16] that it is to the simpler bacteria we must look for the first beginnings of life.

After this digression on the botanical side we must return to the primitive cœlenterate and see on what lines evolution proceeded in the animal world. As a purely plankton organism, swimming freely in the water, the progress of the cœlenterate was not great, and reached, as far as we know, no further than the modern Ctenophore. The Ctenophore seems to represent the culminating point of the primary progression of pelagic animals, which derived directly from the autotrophic flagellate. Further evolution was associated with an abandonment by a cœlenterate-like animal of the pelagic habit, and the establishment of a connection with the sea bottom, either by fixing to it, by burrowing in it, or by creeping or running over it. At a later stage many of the animals which had become adapted to these modes of life developed new powers of swimming, and thus gave rise to the varied pelagic life which we find in the sea to-day; but this must be regarded as secondary, the primary pelagic life, so far as adult animals were concerned, having ended with the evolution of the Ctenophore.[17] Such is the teaching of embryology, the history of the race being conjectured from the development of the individual. In group after group of the animal kingdom, when the details of its embryology become known, the indications are the same—first the active spermatozoon, reminiscent of the plankton flagellate, then the pelagic larval stage, recalling the cœlenterate, and then a bottom-living phase.

[15] Church, *Botanical Memoirs*, No. 3. Oxford, 1919.

[16] Osborn, 'The Origin and Evolution of Life,' 1918. Waksman and Joffe, 'Micro-organisms concerned in the Oxidation of Sulphur in the Soil,' *Journal of Bacteriology*, VII. 2, March 1922. The authors claim that *Thiobacillus thiooxidans* will grow in solutions containing no organic matter. In view of the minute traces of organic matter that suffice for the growth of bacteria and moulds, care must be taken, however, in drawing conclusions from experiments made in flasks or tubes closed in the ordinary way with cotton-wool plugs and subsequently sterilised in flowing steam.

[17] There is perhaps a possibility that further knowledge of the embryology of Sagitta and its allies might make it necessary to modify this suggestion.

The primitive, free-swimming cœlenterate, adopting a fixed habit and becoming attached mouth upwards to solid rock or stone, gave rise to hydroids, anemones and corals, typical inhabitants of the coastal waters, for the sands and muds at greater depths offered few points of attachment sufficiently stable.

A Volvox-like colony of simple holozoic flagellates, according to MacBride,[18] commenced to feed upon miscroscopic organisms lying on the sea bottom, and under these circumstances only the cells of the lower half of the colony would be effective feeders. The upper cells, therefore, lost their flagella and became merely a protective layer, which finally grew downwards outside the others and fixed the colony to the ground. In this way a sponge was formed. The collar cell, so typical of the group, had been developed already by the flagellates, its first inception being perhaps a circle of protoplasmic hairs such as we find in Pedinella. But this adoption of a fixed habit, as it were mouth downwards, did not lead very far, and though there has been much elaboration within the group itself, the sponges have remained an isolated phylum, unable to develop into higher forms.

It is in a Ctenophore-like ancestor that we find the line of development to higher animal groups, and this ancestor must have been at one time widely distributed in the seas. Its immediate descendants are familiar to every zoological student in the well-known series of pelagic larval forms. Müller's larva, taking to the bottom, and in its hunt for food gliding over hard surfaces with its cilia, led to the flatworms; the Pilidium, developing a thread-like body and creeping into cracks and crevices to transfix its prey, gave rise to the nemertines. A Trochophore, burrowing in soft mud and sand, developed a segmented body which gave it later the power of running on these soft surfaces, and became an annelid worm. Another Trochophore, developing a broad, muscular foot, crept on the sand, and afterwards buried itself beneath it as a lamellibranchiate mollusc, or migrated on to harder surfaces as the gastropod and its allies. Pluteus, Bipinnaria, Auricularia, first fixing, as the crinoids still do, and developing a radial symmetry, afterwards broke free and wandered on the bottom as sea-urchin, star-fish and cucumarian. Tornaria developed into Balanoglossus, whose structure hints to us that the ascidians and vertebrates came from a similar stock. All the phyla thus represented derive directly from the free-swimming Ctenophore-like ancestor, and only one considerable group, the Arthropods, remains unaccounted for. The evolutionary history of an Arthropod is, however, not in doubt. Its marine representatives, the Trilobites and Crustacea, came directly from annelids, which, after their desertion of a pelagic life to burrow in the sea-floor and run along its surface, again took to swimming, and not only stocked the whole mass of the water with a rich and varied life of Copepods, Cladocera and Schizopods, but gave rise to Amphipods, Isopods, and Decapods, groups equally at home when roaming on the bottom or swimming above it.

Another important addition to the pelagic fauna we should also

[18] *Text-books of Embryology. Invertebrata.* London, 1914.

notice here. From the molluscs, creeping on solid surfaces, sprang a group of swimmers, the Cephalopods, which have grown to sizes almost unequalled amongst the animals of the sea.

All these invertebrate phyla had become established and most of them had reached a high degree of development in the seas of Cambrian times. Amongst animals then living there are many which have survived with little change of form until to-day. One is almost tempted to suggest that the life which the sea itself could produce was then reaching its summit and becoming stabilised. Since Cambrian times geologists tell us some thirty million years[19] have passed, a stretch of time which it is really difficult for our imaginations to picture. During that time a change of immense moment has happened to the life of the sea; but if we read the signs aright, that change had its origin rather in an invasion from without than in an evolution from within. From whence came that tribe of fishes which now dominates the fauna of the sea? It would be rash to say that we can give any but a speculative reply to the question, but the probable answer seems to be that fishes were first evolved not to meet conditions found in the sea, but to battle with the swift currents of rivers, where fishes almost alone of moving animals can to this day maintain themselves and avoid being swept helplessly away.[20] It was in response to these conditions that elongate, soft-bodied creatures, which had penetrated to the river mouth, developed the slender, stream-lined shape, the rigid yet flexible muscular body, the special provision for the supply of oxygen to the blood to maintain an abundant stock of energy, and all those minute perfections for effective swimming that a fish's body shows. The fact that many sea fishes still return to the rivers, especially for spawning, supports this view, and it is in accordance with Traquair's classical discoveries of the early fishes of the Scottish Old Red Sandstone, which were for the most part fresh- and brackish-water kinds.

Having developed, under the fierce conditions of the river, their speed and strength as swimmers, the fishes returned to the sea, where their new-found powers enabled them to roam over wide areas in search of food, and gave them such an advantage in attack and defence that they became the predominant inhabitants of all the coastal waters, and as such they remain to-day.

The other great migration of the fishes, also, the migration from the water to the land, giving rise to amphibians, reptiles, birds and mammals, must not be left out of account. The whales, seals and sea-birds, which after developing on land returned again to the waters and became readapted for life in them, are features which cannot be neglected.

And so we are brought to the picture of life in the sea as we find it to-day. The primary production of organic substance by the utilisation of the energy of sunlight in the bodies of minute unicellular plants, floating freely in the water, remains, as it was in the earliest times, the feature of fundamental importance. The conditions which control this production are now, many of them, known. Those of chief import-

[19] Osborn, *Origin and Evolution of Life*, 1918, p. 153.
[20] Chamberlin, quoted in Lull, *Organic Evolution*, New York, 1917, p. 462.

ance are (1) the amount of light which enters the water, an amount which varies with the length of the day, the altitude of the sun, and the clearness of the air and of the water; (2) the presence in adequate quantity of mineral food substances, especially nitrates and phosphates; and (3) a temperature favourable to the growth of the species which are present in the water at the time. Experiments with cultures of diatoms have shown clearly that if the food-salts required are present, and the conditions as to light and temperature are satisfactory, other factors, such as the salinity of the water and the proportions of its constituent salts, can be varied within very wide limits without checking growth. The increased abundance of plankton, especially of diatom and peridinian plankton, in coastal waters and in shallow seas largely surrounded by land, such as the North Sea, is due to the supply of nutrient salts washed directly from the land by rain or brought down by rivers. An exceptional abundance of plankton in particular localities, which produces an exceptional abundance of all animal life, is also often found where there is an upwelling of water from the bottom layers of the sea. These conditions are met with where a strong current strikes a submerged bank, or where two currents meet. Food-salts which had accumulated in the depths, where they could not be used owing to lack of light, are brought by the upwelling water to the surface and become available for plant growth. The remarkable richness of fish life in such places as the banks of Newfoundland and the Agulhas Banks off the South African coast, each of which is the meeting-place of two great currents, is to be explained in this way.

Our detailed knowledge of the steps in the food-chain from the diatom and peridinian to the fish is increasing rapidly. The Copepod eats the diatom, but not every Copepod eats every diatom; they make their choice. The young fish eats the Copepod, but again there is selection of kind. Even adult fishes like herring and mackerel, which were formerly supposed to swim with open mouth, straining out of the water whatever came in their way, are now thought largely to select their food.[21]

A result of extraordinary interest in connection with the food-chain has recently been brought to light by two sets of investigators working independently. In seeking to explain certain features which he had found in connection with the growth of the cod, Hjort[22] undertook a study of the distribution in marine organisms of the growth stimulant known as vitamin. Fat-soluble vitamin was already known to be present in large quantities in cod-liver oil, and is what probably gives the oil its medicinal value. Hjort was able to trace the vitamin, by means of feeding experiments on rats, in the ripe ovaries of the cod, in shrimps and prawns, which resemble the animals on which the cod feeds, and in diatom plankton and green algæ. Jameson, Drummond, and Coward[23] cultivated the diatom *Nitzschia closterium*, and by a similar method to that used by Hjort showed that it was extraordinarily

[21] Bullen, *Journ. Mar. Biol. Assoc.*, 9, 1912, p. 394.
[22] *Proc. Roy. Soc.*, May 4, 1922.
[23] *Biochemical Journal.* 1922.

potent as a source of fat-soluble vitamin. We thus conclude that this substance, so essential to healthy animal growth, is produced in large quantities by plankton diatoms, and passed on unchanged to the fish through the crustaceans which feed on the diatoms. In the fish the vitamin is first stored in the liver, and with the ripening of the ovary passes into the egg, to be used to stimulate the growth of the next generation. Again we see the fundamental importance of the food-producing activities of the lowest plant life.

Attention has already been drawn to the suggestion that fishes developed their remarkable swimming powers in rivers, in response to a need to overcome the currents, and that they afterwards returned to the sea, where they preyed upon a well-developed and highly complex invertebrate fauna already fully established there. Their speed enabled them to conquer their more sluggish predecessors, whilst they themselves were little open to attack. With the exception of the larger cephalopods, which are of comparatively recent origin, and were probably evolved after the arrival of the fishes, there are few, if any, invertebrates which capture adult fishes as part of their normal food. Destructive enemies appeared later in the form of whales and seals and sea-birds, which had developed on the land and in the air.

And now in these last days a new attack is made on the fishes of the sea, for man has entered into the struggle. He came first with a spear in his hand; then, sitting on a rock, he dangled a baited hook, a hook perhaps made from a twig of thorn bush, such as is used to this day in villages on our own east coast. Afterwards, greatly daring, he sat astride a log, with his legs paddled further from the shore, and got more fish. He made nets and surrounded the shoals. Were there time we might trace step by step the evolution of the art of fishing and of the art of seamanship, for the two were bound up together till the day when the trawlers and drifters kept the seas for the battle fleet.

There can be little doubt that in European seas the attack on the fishes in the narrow strip of coastal water where they congregate has become serious. A considerable proportion of the fish population is removed each year, and human activity contributes little or nothing to compensate the loss. We have not, however, to fear the practical extinction of any species of fish, the kind of extinction that has taken place with seals and whales. Fishing is subject to many natural limitations, and when fishing is suspended recovery will be rapid. There is evidence that such recovery took place in the North Sea when fishing was restricted by the War, though the increase which was noted is perhaps not certainly outside the range of natural fluctuations. Until the natural fluctuations in fish population are adequately understood, their limits determined, and the causes which give rise to them discovered, a reliable verdict as to the effect of fishing is difficult to obtain.

If such problems as these are to be solved the investigation of the sea must proceed on broadly conceived lines, and a comprehensive knowledge must be built up, not only of the natural history of the fishes, but also of the many and varied conditions which influence their lives. The life of the sea must be studied as a whole.

HUMAN GEOGRAPHY: FIRST PRINCIPLES AND SOME APPLICATIONS.

ADDRESS TO SECTION E (GEOGRAPHY) BY

MARION I. NEWBIGIN, D.Sc. (Lond.),

PRESIDENT OF THE SECTION.

In his address to this Section in Edinburgh last year, my predecessor, Dr. Hogarth, devoted some time to a consideration of the position of geography in the Universities of this country. He had no difficulty in showing that, from various points of view, this position still leaves much to be desired. My present concern, however, is not with the actual facts, but with a deduction which naturally follows from them. If it be true that the Geographical Departments of the Universities are, in most cases, insufficiently staffed and equipped, then it is surely clear that, despite all the progress which has been made in recent years, we have largely failed to convince the great mass of educated opinion of the value of our subject; for University chairs are only endowed, and departments equipped, when those established in educational high places realise the growing importance of the subject concerned. Usually, also, before that realisation can take place there must be a driving force in the shape of a body of enthusiasts, able and willing to convince the general public that the advance is necessary in the interests of the community.

Now, in the case of geography the body of enthusiasts does exist; where we have failed, as I think, is in making continued and determined efforts to convince others. The time seems to me to have come for a determined missionary effort, a deliberate attempt to make clear to the ordinary citizen that geography, in its modern aspects, is a subject of direct interest and value to him in his daily life.

Let me take first a single minor example of the need for such a policy. All those who have had anything to do with the arranging of lecture programmes for geographical societies are aware how largely accounts of exploration bulk in these. It may be said generally that any Committee meeting for such a purpose turns first to a consideration of what returned explorers are likely to be available at the time. More than this, whether geographers in the technical sense are well represented on such bodies or not, there is a general consensus of opinion that an explorer who has come through great dangers, or shown conspicuous personal courage, is, for a society which depends on public support, a much more valuable lecturer than one who has merely done careful and painstaking work, with no element of drama in it.

This means that even that section of the public sufficiently interested in geography to join a geographical society regards the subject as

primarily concerned with exploration, leading to the description of unknown or little-known regions of the earth. Even so, its interest requires stimulation by the personal factor. If this be the attitude of a somewhat specialised public, what is that of the world outside?

I do not think there can be much doubt as to the answer. In so far as that public is highly specialised and consists of students either of those separate sciences from which geography obtains much of its material, or of such subjects as history in its different branches, it tends in many cases to regard geography with tolerant contempt. Of the unspecialised public it may be said generally that the subject in its modern developments has scarcely come within its range of vision. Its older members, especially, are for the most part convinced that they learnt 'geography' at school, as they learnt reading, writing, and arithmetic there, and that, since mountains and rivers, capes and bays and the rest remain where they were, there is little left to be studied or investigated.

It seems to me, therefore, that the most clamant need at the present time is a continuous attempt to make it plain to the community at large that the main interest of geography is not in its facts as such—for if geography ceased to exist the geologists, meteorologists, botanists, zoologists, and so forth would continue to collect most of these. Rather does it lie in the way in which the geographer studies these facts in their relations to each other and to the life of man. Further, whatever place the study of the human response to the surface phenomena of the earth should take in the subject considered as a whole—and the topic was fully discussed by Dr. Hogarth last year—there can be no doubt that it is the aspect which makes the widest appeal. When, for example, we can take the sheets of a good atlas of physical geography and show that the facts represented there can be made to yield deductions of great interest and value to everyone, then we are going far to persuade the members of the public of the importance of geography; and not until they are so persuaded can we hope that the subject will obtain in the higher institutions of learning the position to which we believe it is entitled.

Now, I am well aware that such deductions have been and are being drawn by geographers, both at home and abroad. But their conclusions have so far reached only a very limited audience. It has seemed to me that an Address to this Section gives an opportunity of discussing certain interesting points of view which do not seem to have been fully treated hitherto. In so far, however, as I am addressing an audience of geographers in the technical sense, I wish it to be clearly understood that what I have to say is to be regarded less as a contribution to geographical science than as an attempt to carry out that forward policy which seems to me essential at the moment. Even if I fail to carry you with me throughout, I may at least hope to stimulate some of you to promote the aim already set forth by other and better methods.

For the reason already given I propose to take certain points in regard to the human response to surface phenomena for special consideration. Now, it is a somewhat curious fact that, although

geographers are agreed that man's intelligence and power of acquiring and transmitting knowledge so differentiate him from animals that it is necessary to distinguish between human geography and animal geography; yet, so far as I am aware, little detailed consideration has been given to the question as to the respects in which his response to environmental conditions differs from that of the animals. This is unfortunate, more especially since, thanks to the biologists, we have a fairly clear idea as to the mechanism of the response in the latter case.

If, for example, we take two familiar animals, such as the rabbit and the common hare, we find that, though belonging to the same genus, and generally resembling each other in structure, they show certain minor differences in bodily form and habits fitting them for the environments in which they respectively live. Thus the long legs of the hare enable it to maintain the swift movements upon which it depends for escape from its foes, while the rabbit, inhabiting sandy uplands instead of open country, finds safety underground, and need only be able to move swiftly over short distances. Similarly, the young of the rabbit, born within the shelter of the burrow, are more helpless than the leverets, brought forth virtually in the open. The biologists are broadly agreed that these differences are an adaptive response to the different environments of the two animals. In explaining the origin of that adaptive response, most of them lay stress on the two factors of fixation to a particular environment and isolation—actual or physiological—within it, so that incipient variations are not swamped by intercrossing.

Now when we turn to look at man, two facts are at once apparent. In the first place, at the present time, he does not appear to respond to environmental influences by adaptive modifications of bodily form. Secondly, there was certainly a time, before he had come fully to his heritage, when he did so respond. We know this because the anthropologists are agreed that while man once ran into a number of species—and of genera—now all living human beings belong to the same species, and even the races show marked signs of being in process of becoming swamped by intercrossing. In other words, there was a time when there was no human geography, when man reacted to the sum total of the conditions as an animal does; but that time appears to have largely passed.

But there is certainly still a human response to environmental conditions. What precise form does it take? To a certain minor extent, apparently as an inheritance from what I regard as essentially the pre-human period, there is a direct structural response. One need only mention the presence of peoples with thin, almost unpigmented skins in Western Europe, and the tendency to increased pigmentation alike as the Tropics and the Poles are approached. But though determined efforts have been made to correlate in detail the physical characters of the great races with the climate and relief of the areas where they are presumed to have originated, most of these correlations remain uncertain and speculative.

Man's real response to the surface phenomena of the earth takes the form of a communal, not an individual response. It is the aptitudes

which the members of a community display, the tools which they use, the kind of knowledge which they accumulate, their modes of organisation, their type of material wealth, their traditions and ideals, which show the environmental imprint most closely, far more closely than the colour of their skins or the shape of their heads.

But when and how did the change in the two modes of response come about? To answer this question let us recall what has been already said as to the importance of fixation and isolation in the case of animals. The surface of the earth is almost infinitely diverse, and what the biologists call natural barriers, the major barriers like deserts, seas and mountain chains, or the minor ones produced by the transition from one type of plant formation to another—*e.g.* from the forested river valley to the grass-covered upland—separate different types of environment, and form obstacles to the distribution of most land animals. There must have been a time when groups of men, no less than the pigs in the forest or the asses on the steppe, were firmly gripped by the physical conditions, were isolated from other groups, forced to become fitted by structure and habit for a particular set of conditions, or to die out. But with his growing intelligence man escaped from this iron grip, learnt to make virtually every part of the surface yield enough for survival, proved capable of overcoming every kind of natural barrier. When this occurred the old mechanism of adaptation largely—though not completely—ceased to work. Evolution then might have ceased also, man might have become specially fitted to no environment because fitted for all, if the factors of fixation and isolation had not, in quite a different fashion, obtained a new hold.

He ceased, save in relatively few parts of the earth's surface, to be a continuous wanderer. He settled down afresh on particular parts of it, and there learnt to use his increasingly complex brain not only in utilising to their full the natural resources, but also in modifying the local conditions so that new resources became available. In other words, I wish to suggest that the cultivation of the soil was the great agent in ensuring the new type of fixation to a particular area which once again made evolution possible. But evolution now took the form of increasing development of communal life, or, in other words, the growth of what we call civilisation is the precise equivalent of specific differences in plant or animal.

Further, just as, in the case of the animal, isolation is necessary before an incipient species can become fixed, so in the case of human communities a measure of protection from the inhabitants of neighbouring areas—a measure, that is, of isolation—is essential before civilisation can develop.

Again, in the case alike of plants and animals we know that where the local conditions are such that the incipient species is limited to a very narrow area, there highly specialised forms of adaptation may occur, as they do, for example, on many islands, or in isolated mountain chains; but that specialised type of development is associated with the loss of the capacity to vary, to acquire adaptations fitting the organism for a wider area. So in the case of human communities, where the isolation is too complete the power of adaptation tends to be lost, and such groups, though their civilisation may, along its own lines,

be of a highly specialised type, are easily overwhelmed when contact with the outside world does occur, just as island animals tend to disappear before introduced forms.

Now with these general statements as starting-point, let us consider some facts in regard to the development of civilisation in Europe and the margins of the adjacent continents.

In this area history has seen three successive great foci of civilisation, each based on well-marked and distinctive geographical conditions. The development of the three types has been successive and not simultaneous, and there has thus been a steady shift in time of the main focus, a shift westward and north-westward. The three types of human societies alluded to are, of course, (1) the river valley type as represented in Babylonia and early Egypt; (2) the Mediterranean type on parts of the seaboard of the Midland sea; (3) the forest type of Europe proper, itself becoming progressively more and more influenced by the greater ocean to the west, so that forest influences have steadily given way to maritime ones.

We have to ask ourselves, then, what effects the factors already considered have had on the origin, growth, and further development, or decay, of each of these three? In other words, what in each case were the geographical causes which first fixed man to a particular area in which he was able to cultivate useful plants? What gave the necessary isolation and safety during the early stages? Finally, to what extent were the conditions such as to give that necessary safety without leading to the loss of the power of continued adaptive modification, as expressed either in the capacity to spread over adjacent areas showing progressively increasing differences, or in that of responding to changes within the home area?

In the case of the river-valley areas, as represented in the Tigris-Euphrates region and the Nile valley, and in that of the Mediterranean seaboard, several geographers, among whom Prof. Myres may be especially mentioned, have discussed the conditions favourable to the early development of civilisation. It is therefore not necessary to consider the geography of these areas in detail. But, beginning with Babylonia and Egypt, I should like to put the causes which seem to me to have promoted fixation quite briefly. Among them we must certainly include the primitive natural resources, scanty though these doubtless were. The birds of the valley marshes, the relatively small number of mammals, the fish of the rivers, must have supplied a certain amount of the animal food. The date palm, in the Tigris-Euphrates areas at least, would, even in its wild state, doubtless yield a fruit of some value in the very early days.

But, as an important factor in the development of cultivation, I would lay especial stress upon the presence of what the botanists call the 'open' plant formation. Native trees, as we know, are very few, the date palm, one of the most characteristic, being strictly limited in distribution by its need for water at the roots. For the greater part of the year the ground between the scattered trees is naturally either devoid of vegetation, or this is represented only by a few desert plants. But after the periodic flooding by the rivers, an abundant growth of vegetation springs up. The plants may be annuals,

whose seeds ripen as the ground dries, and lie dormant till moisture comes again; or they may be bulbous and tuberous forms, having but a short period of vegetative activity, but possessing underground stems capable of withstanding prolonged drought. The result is that man did not require to clear land for crops, Nature periodically cleared it for him. He had but to make the fairly obvious deduction that water alone was necessary for the apparently barren soil to blossom like the rose, and from all the choice of plants which the flooded ground offered, to pick out those of some use to him, and learn to suppress the rest. As has often been pointed out, he did not need to trouble greatly about renewing the fertility of his lands, for the flood-water did this for him.

So soon as he had learnt the initial lessons of cultivation, he was tied to the area normally flooded at certain seasons, or to which he could lead the flood-water. He intercalated his crops along one of Nature's lines of weakness, in a transitional area which passed periodically from one climatic zone to another, being, according to the seasons, either a desert or fertile. Fixed in this fashion he could, and did, adapt his mode of life to the natural conditions as precisely as ever bird or insect became structurally fitted for life on an island.

The bordering desert ensured isolation, and, continuing the island metaphor, we may say that it represented the sea. Its effect was to throw the whole energy of the community towards the centre, for the periphery formed an area in which the characteristic mode of life could not be practised. Similarly, it gave protection, for it is unsuited to any save a highly specialised culture, which must have been of relatively late origin. So far as it formed the boundaries of the incipient state, therefore, the desert constituted a barrier preventing the ingress of potential foes. In neither case, of course, was the desert rim complete, and the conditions upstream varied in the two areas, and were, as has been often pointed out, from the point of view of safety, on the whole less favourable in the case of Babylonia than in that of Egypt.

As to the third point, it is, I think, easy to show that while the isolation of the areas was markedly conducive to the rise of civilisation and to its growth up to a certain point, in the long run it became a danger. In the first place, the contrast between the belt which could be watered and that to which, with the means available, water could not be carried, was exceedingly sharp. There was little possibility of a gradual spread into areas becoming slowly but progressively different, where new aptitudes could be acquired, new experience gained, and new forms of wealth stored. Specialisation was high within the favoured tract, but the limits set by Nature could not be passed.

Again, as has often been noted, the conditions led necessarily to a centralised and imperialistic form of social organisation. If there was a sharp line of demarcation between the areas which could and could not be watered, there were great possibilities in the direction of extending by artificial means the belt over which the flood-water spread. This involved the gradual growth of an elaborate irrigation system, and for the maintenance of this a centralised power was essential. This brought with it, as a correlated advantage, the possibility of organised defence when developing neighbouring communities attempted to

encroach. But if the attack was made with sufficiently powerful forces, the centralisation became a menace. An attacking foe able to destroy or damage seriously the irrigation system could cut off at its source the basis of prosperity, and render reconstruction on the old scale almost impossible. In other words, the community became adapted to artificial conditions created by itself; if and when those conditions were destroyed, the survival of the old culture became impossible.

Turn next to the Mediterranean region, that is to the area in which the typical Mediterranean climate prevails. In so far as the native plants are concerned, this area shows certain broad general resemblances to the river valleys, with some striking differences. Thus the characteristic plant formation is alternately open and closed; closed during the cooler season of the year when the winter rains cause a brief but intense growth of annuals and bulbous or tuberous plants, open during the drought of summer when the trees and shrubs stand apart from each other with bare earth between. But the contrast is due, as indicated, to the rainfall conditions, not to flooding. There is thus no natural renewal of fertility, and plants which require much water can only thrive in the cooler season, so that growth is less intense than in either the Nile or the Euphrates-Tigris valley.

On the other hand, because of the climatic conditions, trees and shrubs, alike as regards individuals and species, are far more numerously represented in the Mediterranean region. Here, however, we come to a very curious fact, which, though it is familiar enough, does not seem to have been considered in all its bearings. This is that, despite the (relative) wealth of native species of shrubs and trees, those which are cultivated seem to have been for the most part introduced. This is apparently true even of the supremely important olive. The tree occurs in the fossil state, and the olivaster of the maquis is believed by many to be truly wild, not feral. Yet it would appear almost certain that the *cultivated* olive was introduced, into Europe at least. The same thing is true of great numbers of other species, and of all the fruit-bearing trees now grown in the area there are few indeed which can be reasonably regarded as having originated there as cultivated forms. Now, the deduction that I would draw is that the Mediterranean area is one in which lessons first learnt elsewhere could be easily practised, but one rendered unsuited by the natural conditions for the taking of the first steps. Putting the point in another way, I would suggest that when we see, in any part of the area, olives or fig-trees rising from above a plot of wheat or barley, we have to say to ourselves that this is an adaptation to a new set of conditions of the *type* of cultivation first practised on any scale in Babylonia or Egypt, olive or fig representing date palm and the accompanying trees, the narrow plot of corn the local modification of the broad fertile fields of the river valleys.

Man was doubtless first attracted to the area, as in the case of the river-valleys, by the natural resources, small though these must have been, even with the addition of the sea fisheries. He became fixed to it when he learnt that the hill-spurs gave safe sites for settlements, while affording easy access to the slopes on which his special form of

intensive cultivation could be carried on. That form, as already suggested, was a derived and not an original one. He replaced the native trees and shrubs by useful cultivated varieties or species, which had, certainly for the most part, originated elsewhere. He intercalated short-lived annuals like corn crops and beans along the line of weakness indicated by the periodic opening and closing of the natural vegetation. But one of his great difficulties was always that the absence of much level land and the climatic conditions rendered the growth of such crops relatively difficult, much more difficult than in the river-valleys.

If we think of the early settlements as showing a general resemblance to the Berber villages of the Algerian Atlas to-day, we realise that they were more or less isolated the one from the other, so that the social polity was of a wholly different type from that existing either in Babylonia or in early Egypt. But, and this seems to me important, although the natural conditions—especially the fact that fertility was limited to certain areas—made a measure of isolation inevitable, yet the sea gave a possibility of free movement in all directions which was absent in the river-valleys. Thus oversea, if not overland, spreading could take place, and the changes in the geographical conditions as the sea is traversed westward are relatively small, not outside the limits of adaptation. Thus we have the spread of the higher forms of Mediterranean culture from the eastern end of the sea towards the west, with the founding of new settlements of generally similar type to the old. Greece could, and did, send daughter colonies to Sicily, and those colonies broadly repeated in their new homes the conditions which they had left in their old. This possibility of free movement brought with it a wider range of adaptability, a constant willingness to profit by new experiences, which has proved of enormous value to the world at large.

But with all its advantages the Mediterranean area, as already stated, had the great disadvantage that bread-stuffs were difficult to produce in quantity. Two methods of getting over that difficulty could be and were practised. For example, the ancient Greeks, having, it would appear, learnt the lesson from the Phœnicians, dared, in course of time, to descend from their hill-spurs to the sea-coast, in order to supplement the scanty resources of their limited lands by sea-trading. After a long interval the mediæval cities, especially of Italy, did the same thing on a greater scale and with the advantage of a wider market. Between the two periods Rome tried the other possible method, that of holding in subjection the areas, outside that of the characteristic climate, which were corn-producing. Her failure was, at least in part, due to geographical causes. The great advantage of the method of sea-trading was the increase in the power of adaptation which it brought, as a result of the continual peaceful contact with other lands and other peoples. The decay of the splendid mediæval cities of Italy came when the Mediterranean ceased to be a great highway of commerce, and the vivifying breezes from the outside world which had swept through it took another course—once again, that is, a civilisation based upon a delicate adjustment to a particular set of conditions fell when those conditions changed.

Let us turn next to the third great area where, comparatively late, a complex civilisation grew up, that of the forest belt of Central and Western Europe. Here the conditions appear relatively so unfavourable that man could scarcely have solved the problem of fixing himself permanently to particular areas, and adapting himself to them, were it not for the help of the experience gained elsewhere. The great agent in transmitting that experience was, of course, first the Roman Empire, and then the Church which was the direct heir of the empire.

The essential difficulty here was that the characteristic plant formation was the closed temperate forest. At first sight there appears to be within it no line of weakness along which cultivated plants can be intercalated, and the establishment of cultivation seems to depend upon the complete destruction of the natural vegetation, involving the slow and peculiarly laborious clearing of the forest. The significance of this is admirably illustrated by Mr. Delisle Burns when, in his 'Greek Ideals,' he contrasts Aristophanes' laudation of the agricultural life in the 'Peace' with that of the free and noble life in the forest as set forth by Shakespeare in 'As You Like It.' In the one case the fig-cakes and the figs, the myrtle and violets by the well, the olives, the beans, the barley and the grapes, the rain which God sends after the sowing, which are the elements in the picture, all speak of man's age-long endeavour to mould Nature; but the merry life under the greenwood tree speaks of a thin scattered population, still finding, in theory at least, that Nature unaltered yields all he needs.

Had the temperate forest been in point of fact as continuous as we are apt to assume, the problem would have been so difficult that the hunter's life in the forest might have lasted much longer than it did. We know, of course, that there were always 'islands' in the sea of green, and of these the most important, from the point of view of the development of cultivation, were the loess areas and the lower uplands, especially those over chalk. In the former case the friable, well-drained soil seems to have carried originally but scanty trees; clearing was therefore fairly easy, and the cleared soil proved exceedingly fertile. In the chalk uplands the local conditions made tree growth difficult or impossible, so that land was again readily available for crops or pasture.

We have, therefore, as our starting-point in this case scattered settlements in the woods—not compact ones like those of the Mediterranean region. In essentials these were doubtless quite comparable to those made by fugitive Serbs in the Shumadja, from which modern Serbia finally took origin, though the first foci were almost certainly the natural clearings already mentioned. As in the case of the Serbs, the basis of life was a combination of pastoral industries and arable farming, the pig being the most important source of animal food, and itself finding most of its food in the woods marginal to the settlement.

As to the next stages, the surrounding wood must be regarded from two points of view. Initially it formed a protection, the protective influence being strongest where the land was ill-drained, owing to the dense thickets which covered the marshy ground. But, in contrast to both the types of region already considered, given the necessary tools for the clearing of the land, the particular type of cultivation

could be extended almost indefinitely on the level, while leaving the woods on the rising ground to supply the necessary fuel, building material, and pannage for the swine. This was a great advantage, but it meant that the necessary protection was soon lost.

Now, in North-Western Europe that protective influence was peculiarly necessary for one geographical reason, as it was on the eastern margin of the continent for another. It was necessary in the west especially, because the sea-coasts, owing to the local wealth of fish, early attracted population. But in many regions those coasts, exposed to the oceanic type of climate in its most pronounced form, were unsuited to cultivation. At the same time, on account of their sheltered inlets, parts of those coasts were well fitted to breed a seafaring folk. Unable, or able only to a very small degree, to supplement their natural resources by cultivation, having at the same time command of the sea, those seafarers tended constantly to raid the painfully cleared and cultivated lands of their more fortunately situated neighbours. These, as many old tales inform us, did, time and again, find their encircling woods a protection. We must suppose, therefore, that the tendency to clear more and more land would be checked by this need for the shelter of the woods.

But it seems to me that we may regard the growth of feudalism, from one point of view, as an adaptive device by which the growing agricultural settlements obtained, at a price, the necessary protection. Feudalism in the form, for example, in which it grew up in England before the coming of the Normans was a means of ensuring the existence of a kind of organisation which permitted clearing of forest land to go on indefinitely, while diminishing the risk of perpetual raiding.

It was also, more especially in Eastern Europe, something more, for it tended to fix the cultivator to the land. The tendency to wander may be said to be almost universal in the case of forest-dwellers carrying on primitive agriculture. Its wide distribution is due to the great difficulty of maintaining there the fertility of the land, more especially when exhausting crops, like the different kinds of grain and flax, are grown. To this day, when we contrast the advanced agriculture of Western Europe with the more primitive type practised in the Eastern part, we have to remember that the Western Europeans have largely evaded their problem by using their easy access to the great ocean to draw upon all parts of the world for feeding-stuffs for their large herds of cattle, and mineral fertilisers for their arable lands. In early days the difficulty of keeping many cattle through the winter scarcity, combined with the merely moderate fertility of the deforested lands, made the restoration of material taken out by the crops a matter of great difficulty, got over by a variety of devices, including, of course, fallowing.

Feudalism helped in the solution of this problem by checking the natural tendency of the cultivator to abandon exhausted lands and move on to new ones. But even apart from this particular device, the problem of maintaining fertility had to be tackled early in the West, because the relief made the forest far less continuous, far less uniform, than in the East. It must have been obvious quite early

that it was not illimitable. Conditions were different in the forest region of the East, where the vast, almost uniform plains, the absence of well-marked relief, the breadth of the continent, made the forest a more permanent, a more unmanageable element than in Western Europe. Here, therefore, we find in suggestive combination two peculiar features. The first is that the wandering instinct, the instinct that brought the Slavs from their eastward forest home far into Central and Southern Europe, still persists. It is said to be quite well marked in parts of Russia, despite all the artificial checks which existed under the old *régime*. Part of the difficulty of the Slav problem also lies in the fact that the effect of the habit of small groups of wandering constantly from one wooded tract to another is written large on the ethnological map.

The second peculiar feature is that feudalism, and feudalism in a very harsh form, survived here far longer than in Western Europe, and in fact, if not in law, had scarcely disappeared when the war broke out. I would suggest that the great significance of this form of social policy here was that it helped to counteract the effects of the natural conditions, that it was fundamentally an artificial device for rendering the population stationary, and enabling it to adapt itself to the local relief and associated phenomena.

Now, whatever its value in earlier days, the present chaos in Eastern Europe shows clearly enough that ultimately it checked social evolution, and became a serious menace. It was fundamentally the erection of an artificial barrier round the rural community, and led to the apparent loss of the power of slow adaptation to changing conditions, alike on the part of the overlords and of the freed serfs.

But in the eastern chaos another factor has to be borne in mind. In the Old Russia, south of the forested area, and extending both into what is and was Rumania, lie the great treeless plains. Parts of these, as the nineteenth century show, are extraordinarily fertile and well adapted for cereal production. But, from the point of view adopted here, they suffer from the enormous disadvantage that there is nothing in the natural conditions to fix their inhabitants to special areas, thus enabling them to acquire qualities fitting them for life there; nothing to give protection from constant inroads from Asia. Literally wastes for long centuries, these plains were for the most part ultimately incorporated in Imperial Russia, and deliberately colonised, often with colonists from a distance. The colonists were brought from areas of other characters, possessed traditions and aptitudes due to long experience of different geographical conditions, and were in the grip of a Government which had itself evolved under those conditions. There was thus no question of the possibility of the evolution of a type of culture bearing the imprint of the local conditions.

In consequence Russia to-day—as well as to some extent Rumania—is faced with a double problem. In both regions parts of the constituent lands are fitted for the mixed cultivation of the forest belt, and in them the old social policy has shown itself unfitted for modern conditions, and a new one has yet to be evolved. Other parts, again, have never developed even an imperfect social policy which was a response to their own local environment. Their apparent prosperity,

till the outbreak of the war, was due to the fact that they were, economically though not politically, of the nature of colonies in relation to the industrialised West, were, fundamentally speaking, the equivalents of Imperial Rome's corn-producing lands in North Africa and the Danubian plains. The chaos in Eastern Europe is thus having a reflex disturbing effect upon the West. The West has lost an important market, but that is perhaps in itself less important than the fact that over a large tract of European land man and his environment have been thrown out of gear, a catastrophic condition which inevitably disturbs equilibrium elsewhere. Just as in the later days of the Roman Empire disturbances in the marginal corn-producing lands shook and ultimately overthrew the centre, so are the centres of Western European civilisation to-day trembling under the impact of shocks emanating from the East. We can well understand, therefore, how it is that there are those who believe that the focus of civilisation is destined to undergo another shift, and that the day of the predominance of North-Western Europe is drawing to a close.

The subject is not one which can be discussed here. But if I may sum up briefly the points I have been trying to make, I would say that the human geographer should have before him a twofold purpose. In the first place he should strive to show that the deductions which the biologists have slowly and painfully laid down in the course of the last sixty years apply, though with an essential difference—which requires careful definition—to the life of man. Second, he should use his precise knowledge of the surface of the earth to work out detailed applications of those deductions. In other words, human geography is the biology of man, and, on account of man's vast power of modifying his environment, necessitates a fuller knowledge of that environment than can be required of the biologist in the narrower sense. Investigations along these lines would, I think, promote greatly the interests of geography as a whole, both by making clear to the general public its value and in justifying that intensive study of the surface relief and the associated phenomena which must always remain its basis.

EQUAL PAY TO MEN AND WOMEN FOR EQUAL WORK.

ADDRESS TO SECTION F (ECONOMIC SCIENCE AND STATISTICS) BY

PROFESSOR F. Y. EDGEWORTH, M.A., F.B.A.,

PRESIDENT OF THE SECTION.

Contents.

SHOULD men and women receive equal pay for equal work? This question is in a peculiar degree perplexed by difficulties that are characteristic of economic science. They arise from the presence of a subjective or psychical element that is not encountered in the purely physical sciences. Outward and visible wealth cannot be quite dissociated from the inward feeling of welfare. But the ideas of welfare —well-being, or satisfaction—are deficient in the simplicity and distinctness which conduce to accurate reasoning. It may be, indeed, that there is something indefinite and metaphysical about certain conceptions which the higher physics now involve. But the practical uses of those sciences are not thereby impaired. Speculations about four-dimensional time-space do not much interfere with the work of the engineer. But the connection of our studies with things higher than material wealth affects injuriously the reasoning even about material wealth. Sentiment exercises a disturbing influence—a disturbance peculiarly to be apprehended in dealing with a question which touches not only the pocket but the home. Nor even when this danger is avoided

does the logic of political economy escape the consequences of its connection with the higher parts of human nature. The most correct and unbiassed economic conclusions are liable to be overruled by moral considerations. This fate, too, is particularly to be apprehended for arguments on the present subject. Guarding against these difficulties, I propose to distinguish and to discuss separately two inquiries into which the proposed question may be subdivided, according as it is referred to external wealth only, or also to the attendant internal feeling of welfare.

2. The disturbing effect of sentiment or prejudice makes itself felt at the very outset of the discussion in the definition of the issue to be discussed. In masculine circles the question is often dismissed with the remark that the work of women never, or hardly ever, is equal to that of men. The truth of this proposition will be considered later (below, 14). Here it is relevant to observe that even if the proposition were true the question would not be stultified. For the term ' equal ' is evidently not to be interpreted, for the purpose of this inquiry, as identical in amount. Equality, as Aristotle says, is of two kinds, numerical and proportional, meaning that the share of A is to the share of B as the claim or worth (ἀξία) of A is to that of B. So when Adam Smith propounds a maxim in the observation of which, he says, consists what is called the equality of taxation, it would be trivial to object that the subjects of the State are not all equal in respect of ability to contribute. Of course he meant, as he says in the context, taxation ' in proportion to their respective abilities '; not implying that the abilities are equal. The question then arises (in economics as well as in politics), What is the criterion of that worth (the ἀξία) which governs distribution, according to which shares are to be distributed? ' Pay in proportion to efficient output,' the phrase used by the War Cabinet Committee on Industry, expresses the meaning approximately. By ' equal efficient output ' may be understood, in the phrase of Dr. Bowley, ' equal utility to the employer.' To the same effect others speak of equal ' productivity ' or ' productive value.' With these phrases there must be understood a certain equality on the side of the employee as well as on the side of the employer or community. Thus, when the Children of Israel were compelled to gather straw in the fields, the bricks which they made might have been of the same utility to the taskmaster as when the raw material was obtained gratis. But if the workers received the same remuneration per dozen of bricks as before, we should not say that, as compared with the former terms, they were receiving equal pay for equal work. Again, there might be nothing to choose from the workers' point of view between carrying a certain quantity of silver or the same weight of lead for the same distance; while the employer or customer might derive a much greater advantage from the transportation of lead than from that of silver. If now the carriage of silver is restricted (by custom, say, or favouritism) to a class defined by some attribute unconnected with the value of their service (uncorrelated with speed, security, punctuality, and so forth), the carriers of lead and silver would not be receiving equal pay for equal work, although each class received a pay

proportional to the utility of its service. In short we must understand with the term 'equal work' some clause importing equal freedom in the choice of work. This condition should include equal freedom to prepare for work by acquiring skill. There are thus presented two attributes: equality of utility to the employer as tested by the pecuniary value of the result, and equality of disutility to the employee as tested by his freedom to choose his employment. These two attributes will concur in a *régime* of perfect competition. For then, theoretically, each employer will apply labour in each branch of his business up to the point at which the return to the unit of labour last applied is equal to the cost of that unit, and the same (*ceteris paribus*) as in all branches of each business. Likewise, in the state of equilibrium which characterises perfect competition the employee cannot better himself by taking the place of another. The question thus conceived may be restated: Should there be perfect competition between the sexes? The question thus put requiring a categorical answer, Yes or No, may be labelled A, to distinguish it from the question of degree, B, which may be asked, if a categorical answer is not forthcoming, namely, What sort or amount of competition between the sexes is advisable?

In the question thus stated equal work is defined objectively by the fact that as between two tasks the worker is indifferent. This fact, like the action or inaction of Buridan's ass, is ascertainable by the senses. But something more than what is given by physical observations seems to be implied in ordinary parlance with reference to our question. Some comparison between the feelings of the workers seems to be implied in statements such as the following: 'The remuneration of the peculiar employments of women is always, I believe, greatly below that of employments of equal skill and equal disagreeableness carried on by men' (J. S. Mill, 'Pol. Econ.' ii., xiv., 5). 'Men and women often work side by side in the same schools; . . . and we are satisfied that the work of women, taking the schools as a whole, is as arduous as that of men and is not less zealously and efficiently done' (Report on Teachers in Elementary Schools, Lond., Cmd. 8939). 'An unfortunate female does not receive for thirteen or fourteen hours' close daily application during six days as much as a man for one day of ten hours' (referring to Philadelphia early last century; cp. Carey, 'Social Science,' vol. iii., p. 385). If equal work is interpreted as equal disutility, in the sense of fatigue or privation of amenity, then equal pay may be interpreted as equal satisfaction obtained from earnings. Equality in this sense is not always predicable of equal external perquisites. It is conceivable, for instance, that a quantity of solid food, or a gaudy livery, might in general have more attraction for one sex than for the other. This second question, which is presented by the subjective interpretation of the terms, like the first, may be subdivided according as (*a*) a categorical answer is demanded, or (*b*) the question is one of degree.

In the first of the two inquiries which have been distinguished we may, if we can, maintain the position assumed by Jevons when he disclaimed any attempt to 'compare the amount of feeling in one mind with that in another,' when he affirmed that 'every mind is inscrutable

to every other mind, and no common denominator of feeling seems to be possible' ('Theory of Political Economy,' p. 15). The second inquiry presupposes the faculty which forms the main theme of Adam Smith's 'Theory of Moral Sentiments,' Sympathy; in addition to the self-interest which is prominent in his 'Wealth of Nations.' The first inquiry belongs to political economy in a strict or 'proper' sense, which we may call pure economics. The second inquiry belongs to political economy in a larger sense, which includes the satisfactions attending the possession and use of wealth—say the economics of welfare. The second inquiry is wider than and comprehends the first; since an increase in welfare is, *ceteris paribus*, apt to attend an increase in wealth. As equality in the first sense, concerned with production only, tends to maximise the national income, so equality in the second sense, affecting distribution, tends to maximise that aggregate of welfare which the utilitarian legislation increases, which wise taxation diminishes as little as possible.

Above both these aims, higher even than economic welfare, is well-being other than economic—moral or spiritual good; a hurt to which may well outweigh a gain in satisfactions less independent of material conditions. But the 'should' in the question with which we started is to be interpreted as referring only to advisability in the first or second sense. The answers to the question thus limited may at least afford materials for the answer to it in all its bearings. For the present I confine myself to the question in its first sense. In a sequel I hope to consider the question in its second sense.

3. To the question (A) whether competition between the sexes should be restricted it may seem sufficient to reply that competition between all classes should be unrestricted. In the immortal words of Adam Smith, 'all systems, either of preference or of restraint, being completely taken away, the obvious and simple system of natural liberty establishes itself. Every man, so long as he does not violate the laws of justice, is left perfectly free to pursue his own interest in his own way, and to bring both his industry and capital into competition with those of any other man or order of men.' This system tends to increase 'the real value of the annual produce of its (the society's) land and labour,' or, as we now say, the national income. It is pointed out by Professor Pigou that, in order to secure a maximum of produce, productive resources must be so distributed that the net product of the unit last applied in each branch of industry—the marginal productivity—may be the same for all branches. To this proximate end *laissez faire* is a means. A maximum of wealth will thus in general be attained by unrestricted competition.

4. But a *maximum* is not always the *greatest possible* value of which a quantity is susceptible. The top of a hillock presents a maximum; but it is not always the highest attainable height. Half-way up Mount Everest is higher than the top of Snowdon. So it may happen that the unrestricted play of competition between short-sighted, self-interested employers and desperately poor workers, though securing a temporary maximum of production, may bring about that *degradation of labour* which the warmest champions of competition have

apprehended; notably Francis Walker (' Wages Question,' ch. v. and 'Political Economy,' Art. 343 *et seq.*). There may occur the 'strange and paradoxical result' described by Marshall (' Principles of Economics,' vi., iii., 8; cp. iv., 1): employers adhering to old methods which require only unskilled workers of but indifferent character, who can be hired for low (time-) wages. Suppose that some doctrinaire despot imbued with misinterpretations of the classical economists as deeply as Lenin with the worst interpretations of Marx' dogmas, should insist on absolutely unrestricted competition (subject only to prohibition of force and fraud). He would rule out *minimum wage* and *standard of life*, and other fine phrases (as he would describe them), which disguise the fact that wages are determined by supply and demand. He would prohibit combinations of workpeople. If such conditions could be enforced there would probably result throughout a considerable part of industry a breakdown, or at least a gradually deepening depression. To this *débâcle* the competition of women would largely contribute. It would be particularly effective owing to three incidents. First, the minimum of requirements for efficiency, of actual as distinct from conventional necessaries, is less for a woman than a man (in the ratio of 4 : 5 according to Rowntree). This circumstance might acquire a dangerous importance in a struggle for bare life, though not of much significance, it may be hoped, in prosperous conditions. Secondly, wives and daughters are apt to be subsidised; and though subsidies do not always lead to the offer of work on lowered terms, this result may be anticipated in the case contemplated. Last, and not least, the woman worker has not acquired by custom and tradition the same unwillingness to work for less than will support a family, the same determination to stand out against a reduction of wages below that standard. Altogether, if we are convinced that some action must be taken to avert the evils which have been glanced at (cp. Marshall, vi., xiii., 12), it seems that our question (A) cannot receive a categorical answer in the affirmative.

5. I dismiss section A with the following cautions: (α) Let us not forget the general presumption in favour of *laissez faire*. It may be true that the top of a hill is not so high as that of a neighbouring mountain. It may be probable that by getting down from the hill and getting up on the mountain we shall ultimately attain a position higher than the hilltop. But the transition, over unknown ground perhaps, is not without danger. For example, many who have left the simple path of Free Trade in order to attain greater prosperity through the protection of infant industries have not bettered themselves. (β) Let us remember that there are limits to the effects of regulation. It is well to prescribe: 'The best way to secure the necessary advances in wages would be to set up Trade Boards for all industries and instruct them to bring minimum wages for men as well as women as soon as possible to a level which would fulfil the conditions indicated above (enabling the man to marry and support a family and the single woman to live in decent comfort). The rise will be made possible by the increase of productivity.' But unfortunately, such is the uncertainty of human affairs, the required increase of productivity does not always

follow the determination of a desirable minimum, as the Australians have lately experienced. In the fixing of minimums, as in the cutting of coats, regard must be had to the amount of material or means available. (γ) In view of the uncertainties attending our course once we leave the obvious and simple system of natural liberty let us advance with great caution. Our motto should be *pedetentim*—testing each foothold before committing ourselves to an irrevocable step; prepared to retract if the ground prove unsafe. An excellent example of the appropriate method is afforded by the English Trade Boards. The Committee to which they owe their institution (1908) recommended that 'Parliament should proceed somewhat experimentally,' that legislation should at first be 'tentative and experimental' (Report on Home Work, 1908, No. xv., 40, 54). The first step having proved encouraging a further step was tried. But that further step having proved unsafe is to be retracted, as recommended by the Cave Committee [Cmd. 1645].

6. B. Under section B, dealing with the question as one of degree, there might perhaps be included the comparative treatment of male and female workers among the classes which shall have been excluded from open competition. Thus, according to Charles Booth's plan of segregating the feckless class who spoil the labour market, his class B, what will be the distribution of work and pay (or should we say rations?) as between the sexes? But such questions belong rather to our less purely economic sequel. In any case I shall not be expected to pronounce on hypothetical cases as numerous as the Socialistic schemes which are in the air. Under head B it must suffice to consider a state of things in which, desperate competition having been somehow ruled out, there remain competitors freed from the deranging effect of extreme poverty and incompetence. The case is that of which Charles Booth said that the 'hardy doctrines' of the individualism system 'would have a far better chance in a society purged of those who cannot stand alone ('Life and Labour,' vol. i., p. 167, ed. 2). Or we may recall Mr. Seebohm Rowntree's distinction between wages below and above his minimum: 'the former should be based on the human needs of the workers, the latter on the market value of the services rendered' ('Human Needs,' p. 120). It is the latter kind of wages only that are now to be considered. Let us simplify the problem by at first (I) abstracting the circumstances of family life, considering the labour world as if it was composed of bachelors and spinsters.

7. I. Competition now being freed, the Smith-Pigou principle (above (3)) resumes its authority. The best results will presumably be obtained by leaving employers free to compete for male or female labour. Thus equal pay for equal work will be secured in our sense of the term; which does not imply that the earnings of the sexes should be equal (2). Equality in our sense would be realised in the conceivable state of things which a high authority (Professor Cassel) appears to regard as actual when he argues that but for the inferiority of female labour 'it is not clear why the employer should not further (than he does) substitute female labour for the dearer male labour' ('Theoretische Sozial-Economie,' p. 293). There is much force in Professor Cassel's argument; and his conclusion would be perfectly true if the implied

premiss, the existence of perfect competition, were true. But competition is not perfect while it is clogged by combinations both of employers and employed. An employer of many workmen is in himself virtually a combination, as Dr. Marshall has pointed out. Men being generally better organised than women have exercised an unsymmetrical pressure on the employer to their own advantage. For instance, 'London printing-houses dare not employ women at certain machines unless they are prepared to risk a long and costly fight' (Mrs. Fawcett, *Economic Journal*, 1904, p. 297, cp. 1892, p. 176). I have been told of similar proceedings elsewhere.

8. The concession of the employer to male pressure is facilitated by the circumstances that, though the use of male labour beyond a certain limit is to his disadvantage, yet it is probably not *very much* to his disadvantage. This circumstance is deducible from a proposition pertaining to the theory of maxima, of which I hereafter shall make much use. It may be stated thus: If y is a quantity which depends upon—increases and decreases with—another quantity, x, the change of y consequent on an assigned change of x is likely to be particularly small in the neighbourhood of a value of x for which y is a maximum. For example, in ascending a dumpling-shaped hill from a point of the plane on which the hill stands, the first hundred yards of advance in the direction of the summit might correspond to an elevation of fifty yards above the plane. But as the summit is approached the same change of length measured along the surface may be attended with a change of height that is a hundred times, or even a thousand times, less than what it was at a distance from the summit. The principle is illustrated by the well-known proposition that a small tax on a monopolised article forms a *very small* inducement to the monopolist to raise the price and reduce the output of the taxed article. Thus, in an example given by Cournot (to illustrate another property of monopoly) a (specific) tax amounting to 10 per cent. of the price before the tax will afford a motive to the monopolist to raise the price, but a very weak motive, since by making the change he will benefit himself only to the extent of $\frac{1}{2}$ per cent. of his profits. A tax of 1 per cent. would afford a very much weaker motive. By raising the price to the figure which (after the imposition of the tax) yields maximum profit he stands to gain (to save upon the loss caused by the tax) about a twenty-thousandth part of his original profits!

9. The pressure of male trade unions appears to be largely responsible for that crowding of women into a comparatively few occupations, which is universally recognised as a main factor in the depression of their wages. Such crowding is *primâ facie* a flagrant violation of that free competition which results in maximum production and in distribution of the kind here defined as equal pay for equal work. The exclusion of women from the better-paid branches of industry may be effected less openly than by a direct veto, such as the 'No female allowed' in the rules of an archaic society ('Industrial Democracy'). Withholding facilities for the acquisition of skilled trades comes to much the same as direct prohibition. A striking instance is mentioned by Mrs. Fawcett with reference to the allegation that women are unable to 'tune' or

'set' the machines on which they work. They were never given the opportunity of learning how to perform these operations (*Economic Journal*, 1918, p. 4). Exclusion may also be effected by regulating that women entering an industry should conform in every particular to arrangements which are specially suited to male workers. Of such rules Mrs. Fawcett has well written, 'to encourage women under all circumstances to claim the same wages for the same work would be to exclude from work altogether all those women who were industrially less efficient than men. A woman who was less capable of prolonged physical toil, who was less adaptive and versatile than the average man, would be forbidden to accept wages which recognised these facts of her industrial existence' (*Economic Journal*, 1894, p. 366; cp. 1904, p. 296). The exclusiveness of male trade unions has been in the past at least fostered by prejudices and conventions that are becoming obsolete. Before the Labour Commission, for instance, a witness was asked, 'What is there unwomanly in steering a barge?' Answer: 'It is a work that is entirely unfit for women'; also 'it reduces the wages of men.' Before an earlier Committee it was testified of another occupation: 'It is most degrading for women . . . it weakens their constitution . . . and not only so, but it is depriving men of their proper labour.' It should be remembered, however, that many of the prohibitions and prejudices here mentioned as contravening free competition were adapted to avert that catastrophic competition (4) which we here conveniently suppose to be excluded.

10. The oppressive action of male unions should be counteracted by pressure on the part of women workers acting in concert. Suppose now that these balanced forces encounter the resistance of the employers, themselves perhaps associated, what will be the resultant? We may assume that the resulting arrangement will not be in strong conflict with the natural forces of competition. Probably an arrangement that the weekly earnings of women should be the same as those of men, though the actual value of a woman as a worker was about 30 per cent. below that of an average man employed in the same capacity (as testified by a majority of employers before a Committee of the British Association, Kirkaldy, 'Credit, Industry, and the War,' 1915, p. 108) could not be maintained without tyranny on a Russian scale. But within limits thus prescribed there is room for a considerable variety of arrangements. On what principle, then, will a more exact determination be obtained? The principle most congenial to the present subsection is that which is suggested by Walker's doctrine, that 'competition, perfect competition, affords the ideal condition for the distribution of wealth' ('Political Economy,' 2nd ed., s. 466; cp. s. 343). We should then not only keep within those limits outside which it would be futile to set up any arrangement, as it would be swept away by the forces of competition, but also within the wide tract thus delimited we should endeavour to find the particular point which would be determined by ideal competition. The first of these precepts may conceivably be carried out by a board of employers and employees. But the second is evidently a counsel of perfection. As Professor Pigou says with reference to railway rates,' it

is plain that anything in the nature of an exact imitation of simple competition is almost impossible to attain' ('Wealth and Welfare,' p. 267 *et seq.*). In the case before us the task of the board would be particularly difficult. For, first, even if the labour contract were of the simplest possible type—so much energy applied, so many foot-pounds raised, in return for so much standard money—it appears from the mathematical theory of demand and supply that, even if competition between employers and employed were as free as can be supposed, a determinate position of equilibrium would not be reached. And the contracts with which we have to do are not simple. As well explained in the First Report on Wages and Hours of Labour (1894, C. 7567) and elsewhere, the wage-rate proper to each kind of work is obtained by numerous extras and deductions corresponding to variations from a standard article or process with specified price—a standard which is itself far from simple. Here, for instance, is, or was, the definition of the standard woman's boot: 'Button or Balmoral, $1\frac{1}{2}$ in., military heel, puff toe; 7 in. at back seam of leg machine sewn, channels down or brass rivets, pumps or welts, finished round strip or black waist.' The extras (and likewise the deductions) may be presumably calculated on the principle described by Mr. and Mrs. Webb as 'specific additions for extra exertion or inconvenience,' so as to obtain 'identical payment for identical effort.' Are these additions, and also the standard to which they are referred, to be determined objectively as what would result from the play of ideal competition? Or must we call in Socialistic, or, as I prefer to say, Utilitarian, principles of distribution in order to fill in the details left blank by the award of competition? However this deep question is decided, it remains true that on the suppositions here made (B I.) the distribution of work and pay between the sexes ought to be conducted upon the same principles as between any other classes of workers.

11. On the general principle of distribution I have nothing to add to the little that I have said here and elsewhere. I subjoin some suggestions for carrying out the principle in the case before us. They relate to the comparative efficiency of the sexes, concerning which assumptions are to be made with caution. There are to be avoided two opposite misconceptions: the one exaggerating the comparative efficiency of men, the other that of women. The first exaggeration is countenanced by Plato when, notwithstanding his admission of women to the highest posts in his Republic, he yet holds that they are inferior to men in all the arts. Even in those arts in which they might be expected to excel, such as weaving and cookery, he seems to say that they are beaten by men. In the modern world, however, it appears that women excel in certain branches of the textile art. 'Having smaller hands they are able to handle the twist and weft with greater dexterity than men' (Cmd., 167, 79). Superiority is claimed for them, too, in typewriting and in telephoning. As nursery-maids they are certainly more efficient. The opposite exaggeration is committed by feminists when they maintain, in the words of a generally impartial expert, that 'there is no reason save custom and lack of organisation why a nursery-maid should be paid less than a coal-miner.' No doubt it is difficult to disprove, and even to define, this proposition with reference to employments that are

not common to both sexes. The comparison would seem to be as to the time-wages, say the average weekly earnings, of the two classes. The institution of the average presents difficulties. Still, I submit it as an inference based on general impressions and ordinary experience that, even if all restriction of the competition between male and female workers were removed, we should still find the average weekly earnings of the former to be considerably higher.

12. The following fuller statement of the matter is submitted as intelligible and probable. Let us suppose at first that work can be defined in such precise and neuter terms that it makes no difference to the employer whether a unit of work is performed by a man or a woman. The definition should include not only a specification of the product, as in the case of the boot above instanced, but also the time taken up (affecting the 'overhead' charge), the expenditure on apparatus (which may be greater for weaker persons), and so forth. In ideal competition men and women shall be equally free to choose any of the occupations so defined. It may be expected that there are some branches of industry into which women only will enter, others into which they will never, or hardly ever, enter. Let us call the former A, B, C, . . . F, and the latter, M, N, . . . Z. Let the average weekly earning in each of the former occupations be a, b, c, . . . f; and in the latter m, n, . . . z. Then I submit that the average of a, b, c, . . . f will be less than the average of m, n, . . . z. There remain occupations that are entered by both sexes: say G, H, I, K, L. For any one of these, I, the (rate of) pay, say ι, for unit of work in the sense above defined is the same for men and women; but the weekly earnings will not be the same, say i_1, for the female and i_2 for the male workers; i_1 less than i_2. The letters may be applied so that f_1, g_1, h_1, . . . l_1 will form an increasing series; on which supposition it may be expected that g_2, h_2, . . . l_2, m_2 will also form an increasing series, rising from the female to the male level.

The conception thus presented may be illustrated by an Australian ruling. Judge Higgins fixed the minimum rate for fruit-picking at one shilling an hour, observing that 'the majority of fruit-pickers are men,' that 'men and women should be paid on the same level,' the employer being left free to employ persons of either sex. But for the operations in the packing-sheds the minimum for (women) workers in these processes, in which men are hardly ever employed, should be fixed at 9*d*. per hour ('Commonwealth Arbitration Reports, 1912,' vol. vi., p. 72, and context). Fruit-picking and the operations in the sheds might correspond to our L and G respectively.

If the rates attached to each specification of work are proper the distribution will be ideal. Suppose that a slightly different system of rates, α', β', . . . ι', . . . μ', ν' . . . &c., is adopted. There will be a slight difference in the distribution of work and pay. But by the property of a maximum above noticed the difference to the community considered as a sort of collective monopolist, the difference to the national income will be not merely slight, but *very* slight.

13. It should be understood that the preceding representation relates only to the present, or rather to a short period in the immediate

future. The period must be long enough for the removal of trade-union restrictions to be realised, for training hitherto denied to be acquired; but not long enough for a material change in physique, arts and customs. If in the course of evolution the female sex became as strong as, or even stronger than, the male, if in the progress of practical science muscular strength became less and less in demand, then the average of a, b, . . . f might no longer be less than the average of m, n, . . . z. Again, a conceivable change in desiderata would affect the truth of our representation; for instance, if type-writing, telephoning and the like became more in demand than coal-mining and ironworks. Again, if the vast amount of household work that is now unpaid could only be obtained by paying for it, the demand for woman's labour and its price might be considerably raised. The general principle of equal distribution above indicated would hold good notwithstanding these changes; but the suggestions made for its working would require modification. The changes, however, do not appear very imminent.

14. Existing institutions being presupposed, it should be noticed that the supposition above made of work defined irrespective of sex is somewhat abstract. It would be appropriate in the Socialist community imagined by Anatole France ('Pierre Blanche'), where the employer would not inquire whether an applicant for work was a man or a woman. He would not be informed by the garments of the applicant, identical attire having been introduced along with equal conditions of work. But in the present state of things it will often be within the knowledge of the employer that it is more profitable to employ a man than a woman, although the work performed by each is identical so far as it can be defined by the most exact rate. For a woman, unlike a man, is 'liable to go off and get married just as she is beginning to be of some use,' as a candid champion of equal pay has observed (*Economic Journal*, 1917, p. 59). Again, a woman is generally less useful in an emergency. As a witness before the Committee on the Employment of Women put it, 'A woman punching a ticket may appear equal to a man, but she is not so useful in case of a breakdown or runaway.' Of course these 'secondary' differences, as they might be called, are much less serious in some industries than in others. In some permanence may be less a desideratum, a breakdown less to be apprehended. Among secondary differences is hardly to be reckoned the alleged inability of women workers to 'tune' the machines on which they work; for that regularly recurring need can be allowed for by a properly constructed rate. But it is otherwise with the risks which hardly admit of actuarial calculation. Besides, even if the probability could be calculated precisely, the compensation to the employer for carrying the risk is not to be measured by the mathematical 'expectation' thereof. This point has been well brought out with reference to risks in general by Mr. Keynes in his great treatise on Probability. The point is of importance here as it contravenes what *primâ facie* seems the simplest solution of the difficulty: that is, in all the industries where secondary differences between the sexes are operative to lower the rates for female work correspondingly. Thus

in industry E, instead of the rate ε which would be proper in the absence of secondary differences, we should put the somewhat lower rate ϵ'. Likewise in I (above (12)), instead of the common rate ι for men and women equally, we should put a lower rate ι' for women, retaining ι for men. Such an adjustment seems to carry out the recommendations of the (majority of the) War Cabinet Committee when they contemplate 'a fixed sum to be deducted from the man's rate' corresponding to the 'lower value of the woman's work,' if proved by the employer (par. 10 (5) p. 4). The adjustment would be in accordance with the definition of equal pay for equal work given by those who are best qualified to interpret the claim: 'Any permanent disadvantage that adheres to women workers as such should be allowed for by a *pro ratâ* reduction in their standard rates' (Mrs. Fawcett, citing Miss Eleanor Rathbone, *Economic Journal*, 1918, p. 3). But the reduction corresponding to the demand of the employer for women as compared with men workers could not well be calculated objectively by a board. It could only be determined by the play of ideal competition, which exists only in idea. There would be incurred the danger either (α) of the women's rate being fixed high above the point for which production would be a maximum, or (β) its being 'nibbled' by the employer. The former danger is probably, as things are, not very serious; the latter is much apprehended by experts. Altogether it would seem better to proceed on the lines of Mrs. Sidney Webb's 'occupational rate,' rather than on the plan recommended by the majority of the Committee. Instead of fixing two rates, ι and ι', let us fix (for the defined unit of work) a single rate for men and women alike, say ι'', less than ι, which would have been the rate in the absence of 'secondary' differences. The readjustment will result in a redistribution of male and female work. The men would back out of occupations in which previously it had been worth their while to take part; the employment of women would be correspondingly extended. The process may be illustrated by an incident which Mr. and Mrs. Webb have recorded. The reduction of a farthing in the pay for a dozen of stockings resulted in that branch of the industry being deserted by the men and occupied by the women ('Industrial Democracy,' II., p. 502). If the reduction, from ι to ι'' was inconsiderable the consequences to the consuming public would be negligible upon the principle above explained (8). Otherwise a great drop from ι to ι'' might have as bad an effect on production as fixing a women's rate, ι', too near ι, the men's rate, so as to incur the danger above labelled α.

15. The specious arrangement by which secondary differences may be masked through the adoption of a uniform rate is not applicable to another kind of difference between the work of the sexes which occurs in the case of some personal services. The vexed question of schoolmasters' pay illustrates this 'tertiary' difference, as it may be called. If teaching were an art as mechanical as turning a prayer-wheel, if teachers were literally, as some of them used to be called, 'grinders,' then (apart from secondary differences) it would be unreasonable that men should be paid more than women for the same operation. But supposing that the presence and influence of a master, say in dealing

with the bigger boys, is something different from that of a mistress, and that it is considered indispensable, it is not unreasonable (in a *régime* of pure economics) that the desired article should be purchased at the market price. The market price of a master is higher if he comes from a class between our M and Z (14), for which the average is higher than a corresponding class of women between A and F. His higher pay is quite consistent with the finding of the teachers above cited (2), that 'the work of women, taking the schools as a whole, . . . is not less zealously and efficiently done than that of men.' They might, indeed, be more diligent and in most branches of education better teachers than men. A steel knife is a more useful implement for general purposes than a silver blade. But if silver is required to preserve the flavour of dessert, the epicure must pay for the metal which has the greater value in exchange. A good cab-horse may, for all that I know, draw a vehicle as well as a high-stepping thoroughbred. But if for purposes of State and show the high-paced animal is required, high prices must be paid for the high paces. The distinction, it will be noticed, turns upon the nature and presence of the horse. If for the carriage of parcels one kind of horse was as efficient as the other, then, indeed, a carrier who charged a higher price for the delivery of parcels because he employed a particular breed of horse could only maintain this differential charge through a, presumably noxious, monopoly. That is the difference between the case of the schoolmistress and the case of Mrs. Jones, whose grievance is recorded by Mrs. Fawcett. Mrs. John Jones during the illness of her husband passed off her own work as his to the firm of outfitters which employed him to braid tunics. 'When, however, it became quite clear, John Jones being dead and buried, that it could not be his work, . . . the price paid for it by the firm was immediately reduced to two-thirds of the price paid when it was supposed to be her husband's'! (*Economic Journal*, 1918, p. 1). Here, in the absence of tertiary (and presumably also secondary) differences, the differentiation of wage was certainly contrary to the principle of equal pay for equal work.

On behalf of the schoolmistresses it may still be urged that the market price of male work is artificially raised by inequitable laws and customs. To this the Teachers' Committee might reply that if the time in this respect is out of joint, they were not created to set it right. But it is here questioned whether the time is so much out of joint. It has been submitted that the average earnings of male labour $(m-z)$ would probably be higher than the female average $(a-f)$, even if there had been introduced the most perfect freedom of competition that is thinkable in the present state of things (12). If so, the higher pay of masters for similar work does not violate the rule of equal pay for equal work in the first, purely economic sense of the rule (2). The unequal pay for equal effort does violate the rule in the second, utilitarian or hedonic, sense. In fact, the instance is well suited to bring into view the essential difference between the two definitions of the formula. The political Socialist who aims at a closer approximation of pay to efforts and needs, the Utilitarian moralist who desiderates, indeed, that ideal, but has regard to the danger of pursuing it too directly, naturally do

not acquiesce in the present arrangements (cp. Report on Women in Industry, Cmd. 135; Minority Report by Mrs. Sidney Webb, sections 12 and 6). But these considerations lie outside pure economics, and must be postponed to our sequel.

16. II. The presumption in favour of free competition and the methods of putting it in practice require to be reconsidered when we restore the abstracted circumstances of family life. We now encounter the dominant fact that men very generally out of their earnings support a wife and family. 'It is normal for men to marry and to have to support families. . . . It is not normal for women to have to support dependants' (Seebohm Rowntree, 'Human Needs,' p. 115). These words express a very general belief and sentiment. It is a norm accepted throughout the civilised world. It is embodied in the Australian determination of minimum wage, one of which, by Judge Higgins, has been above cited (12). Another Australian Judge rules: 'The man, and not the woman, is typically the breadwinner of the family' ('South Australian Industrial Reports,' vol. ii., 1918-19). Justice Jethro Brown grounds an award on 'the traditional social structure which imposes on men the duty of maintaining the household.' So Professor Taussig, 'For a man wages must normally be enough to enable a family to be supported and reared. The great majority of working women are not in this case' ('Principles,' ch. 47, s. 9, vol. ii., p. 144). It cannot be supposed that these authoritative expressions of belief have no correspondence with reality. Indeed, the wiser and more moderate advocates of equal pay for women admit it to be 'unlikely that any large proportion of married women will aim at earning their own living as the norm or standard' (Miss B. L. Hutchins, 'Conflicting Ideals,' p. 63). Few would agree with the authoress of 'A Sane' (*sic*) 'Feminism' that 'domestic morality and feminine dignity make it essential for the married woman of to-morrow to be independent of her husband's income, and therefore normally dependent on some occupation outside the home, . . . a work to be continued throughout married life, with occasional lapses incidental to child-bearing' (pp. 111, 113). Even Mill admits that 'in an otherwise just state of things it is not . . . a desirable custom that the wife should contribute by her labour to the income of the family . . . the actual exercise in a habitual or systematic manner of outdoor occupations, or such as cannot be carried on at home, would . . . be practically interdicted to the greater number of married women' ('Subjection of Women,' pp. 88-89). Does it not follow that the husband must support the family, so far as he is not assisted by contributions from adult children or the occasional—not 'systematic'—work of the wife?

17. It has been sought to evade this stubborn fact by the contention that the occupied single woman is responsible for the support of as many dependants as the man. On the strength of an investigation conducted by the Fabian Research Committee it is maintained that 'two-thirds of the wage-earning women are not only entirely self-supporting, but have others to maintain besides themselves.' But grave doubts are thrown upon these figures by the more elaborate investigation which Mr. Seebohm Rowntree has recently conducted. He finds from an

extensive observation of samples that 'only 12.06 per cent. of women have either partially or entirely to support others beside themselves' ('Responsibility of Women Workers,' p. 36). If we except the cases due to the death of 'the normal breadwinner'—admittedly requiring special treatment—the proportion is reduced to 4.12 per cent. The figure would not be serious even if it proved on further inquiry to be somewhat greater. For the figure has not the same significance as that which relates to the dependants of the male wage-earners. The sustentation of the old and infirm cannot be compared, as regards at least economic importance, with the support of the young, the cost of which normally falls on the male breadwinner. The world got on tolerably before the institution of Old Age Pensions; but it could not have got on at all without the support of young children by their fathers.

18. If the bulk of working men support families, and the bulk of working women do not, it seems not unreasonable that the men should have some advantage in the labour market. Equal pay for equal work, when one party is subject to unequal deductions from his pay, no longer appears quite equitable. It is hardly to be expected that the representatives of female interests should look at this question from the masculine point of view. The ladies who have shown this unusual degree of sense and sympathy are entitled to a very attentive hearing. Miss B. L. Hutchins, in her 'Conflict of Ideals,' has discerned with remarkable insight the antithesis between the traditional status of the husband and father, expected to support a family, and the modern *régime* of contract tending to universal competition. Miss Hutchins does not see her way to ending the conflict: 'it is almost impossible to make any logical scheme or theory that will fit the woman and the young child exactly into a commercially organised society based on exchange values' (*loc. cit.*, p. 69). Miss Eleanor Rathbone, equally discerning the difficulty, is more confident about the solution. She proposes a scheme which has certainly the merit of being logical, the endowment of motherhood, as set forth in her article on the 'Remuneration of Women's Services' in the *Economic Journal* for 1917. The plan deserves consideration here as a step towards that freedom of competition which has been prescribed. The plan may also be advocated as conducing to advantages less purely economic than those now considered. When those other advantages come to be thrown into the scale, the weight of the economic arguments which I now attempt to estimate will still be a relevant datum.

As text of the plan to be examined we may take the pamphlet entitled 'Equal Pay and the Family,' the report of the Family Endowment Committee formed in 1917 at the suggestion of Miss Rathbone. With this pronouncement should be placed the proposal independently made by Mrs. Sidney Webb in her evidence before the War Committee (1919, Cmd. 135). The bright and clear *résumé* of the arguments given by Mrs. Stocks in the booklet entitled 'The Meaning of Family Endowment' is also to be considered.

The purpose of the scheme may be summarised in the words of the Endowment Report: to secure 'that *within each class of income* the man with a family should not be in a worse position financially because he has a family than the single man *in that class*.' For the partial

attainment of this purpose, allowances for children being paid only for six years, there would be required an annual grant of 154,000,000*l*. For the fuller realisation of the plan, continuing allowances for children up to the age of fifteen, the cost would be 240,000,000*l*. (*loc. cit.*, p. 44). 'Something like 250 millions sterling annually' is the estimate of Mrs. Sidney Webb (*loc. cit.*, p. 307).

Let us separately consider, firstly the advantages, secondly the disadvantages, which this plan presents, and, thirdly, whether there is any alternative course by which much of the good result with little of the evil may be obtained.

19. i. One main advantage is thus stated in the Endowment Report: 'When the national endowment of mothers and children becomes an accomplished fact this excuse for the under-payment of women (that men have families to keep) will no longer hold good and women will be free to claim—and men to concede to them—whatever position in industry their faculties fit them for, at a wage based on the work they do, and not on their supposed necessities' (p. 18). The endowment 'would do away with the present involuntary blacklegging of men by women, by depriving employers of their one really plausible, if not actually valid, excuse for paying women less than the standard rates; so putting the competition between the sexes for the first time on a basis which is at once free and fair.' The endowment would certainly facilitate the adoption of that free and fair competition which has been above recommended (9). But that recommendation presupposed that there had been ruled out a sort of competition which is described by some high authorities as not free, which is at any rate generally regarded as deleterious. That tendency to the degradation of labour is, as above explained (4), aggravated by the competition of women. Now the endowment of motherhood would not suffice to remove this danger. The transitory and episodical character of female labour would still threaten male wages. It may be objected that men, freed from the obligation of supporting a family, would no longer have a reason for not competing *à outrance* with equally free women. They might not have any reason; but they would surely long retain the habit, the 'social custom' as it has been called, engendered by their traditional position as at least potential heads of families. In short, the proposed endowment would not remove all the difficulties attending competition between the sexes, but only those attending the ordered competition for which alone I venture to prescribe (Class B above). How large an endowment would be required to counteract the consequences of removing the restrictions on female competition? A measure is afforded by the extent to which male wages would be depressed. In making this computation we may, I think, omit to take account of wives' earnings. For if, on the one hand, the greater efficiency, and possibly the greater number, of married women competing in the labour market tend to depress male wages, on the other hand there is a countervailing gain to the family. We need only, then, consider how much male wages are likely to be diminished by the liberated competition of spinsters. In making this estimate we have to take into account the elasticity of labour, the probability that the greater supply of work

will be met by a corresponding demand for work. We have to take into account also the probability above suggested (12), that the demand for goods in the production of which men's labour plays a great part greatly exceeds, and will continue to exceed (13), the corresponding demand for women's work. When these two circumstances are taken into account it may be doubted whether any great reduction of male wages would follow on the improvements suggested—better training of women, hours and appliances suited to their requirements, in short every degree of freedom that does not evidently tend to the degradation of labour. A comparatively small endowment, then, might suffice to deprive men of a reason for objecting to free competition. The excuse, indeed, without the reason might remain. And no doubt the more completely that the burden of supporting a family is taken off the shoulders of men, the more effectually will the excuse be stopped. But a reason more specious than stopping an excuse may be advanced in favour of a large endowment. If we are about making an endowment, why confine ourselves to the one advantage of smoothing the way for free competition? Let us take the opportunity of securing a second advantage.

ii. The second advantage is the possibility of distributing the resources available for the nurture of children in such wise that the requirements of the larger families may be met more adequately than on the present system. This advantage is thus forcibly stated by Mrs. Sidney Webb: 'In the actual course of Nature the distribution of children among households varying from none to a dozen or more; the number who are simultaneously dependent on their parents varying from one to more than half-a-dozen; and the time in each family over which this burden of dependent children extends, varying from a year or two to ten times that period—bear, none of them, any relation to the industrial efficiency either of the father or of the mother; or to the wage that either of them, or both of them, could obtain through individual bargaining by the higgling of the market; or yet to any actual or conceivable occupational or standard rates to be secured by them, either by collective bargaining or legislative enactment' (Report of the War Cabinet Committee [Cmd. 135], p. 306). By a children's allowance payable to the mothers in all the households of the United Kingdom it may be secured that 'adequate provision is made for children not by statistical averages, but case by case.' This second advantage, as well as the first, would certainly be considerable, if it were unmixed.

20. I will now enumerate some disadvantages; in no particular order, seeing that the relative importance of the objections will not be the same for different mentalities.

i. To some the Socialist character of the scheme will form a prime objection. The increase of bureaucratic routine, the deadening of individual initiative, will be apprehended.

ii. The end proposed by the Socialist is commendable: to give the hungry a larger share of the good things produced by the community. But if the grain which would have been sown for the harvest of next year is used to fill the hungry with cakes this year, the participants of future harvests may be worse off than they would have been if the

resources of the provident had not been thus applied. It requires the subtlety of a Pigou to devise transferences from the rich to the poor which shall not have the effect of curtailing the national dividend (cp. 'Economics of Welfare,' pt. v., ch. ix., ss. 7, 8). But there is reason to apprehend that no such subtlety would be exercised in the case before us. The Endowment Committee touch lightly the question of finance. They mention as an alternative to income-tax a levy of so much per cent. on all incomes, including those of the class not paying income-tax. But is it likely that this method will be employed? Mrs. Sidney Webb thinks it better that the children's fund should be 'provided from the Exchequer (that is to say, by taxation, like any other obligation of the community)' (*loc cit.*, p. 309). No doubt a graduated income-tax would play a great part in the formation of the fund. Much of the popularity which the scheme enjoys in labour circles is probably due to the prospect of transferring hundreds of millions from the income-tax-paying classes to the families of working-people. The imposition of an enormous additional burden on the former class would surely tend to check saving.

iii. The scheme would resemble the quality of mercy in having an effect both on him that gives and him that takes. But the resemblance would end there. The effect on the contributor will be depressing, but the effect on the recipient is likely to be more seriously deleterious. It does not require much knowledge of human nature to justify the apprehension that in relieving the average house-father from the necessity of providing necessaries for his family you would remove a great part of his incentive to work. There is doubtless much exaggeration in evidence which has been given to the effect that when wives earn husbands idle. Yet there is probably an element of truth in the saying which is thus reported by one of our most experienced lady-inspectors, 'I almost agree with the social worker who said that if the husband got out of work the only thing that the wife should do is to sit down and cry, because if she did anything else he would remain out of work' (Report on Home-Work, 46, Question 1027, cp. 1024-5). A gratuitous allowance to the mother would have an effect in this direction at least as great as her earnings have. A homely truth is expressed by Rudyard Kipling with his usual vigour when he describes how the workmen, at the Congress convened by 'Imperial Rescript,' received the invitation to adopt Socialistic motives:

'To ease the strong of their burden, and help the weak at their need.'
The English delegate replies, 'I work for the kids and the missus'; and the workers of all countries join in declaring,

'We will work for ourselves and a woman for ever and ever. Amen.'
I owe this quotation to Mrs. Fawcett, who has used it with effect in the course of a powerful protest against a scheme similar to that now under consideration, proposed by a member of the Endowment Committee (*Economic Journal*, 1907, pp. 377-8).

It may be urged that similar objections were made to Old Age Pensions, which yet have proved a success. But the motives affected by pensions given to parents were not exactly the same as those now considered; the very mainspring of industry was not equally touched.

Nor was the measure so tremendous a step in the dark. The initial cost of Old Age Pensions was but a twentieth part, and the present cost is but a tenth part, of the colossal sum demanded for the endowment of motherhood.

iv. It will be gathered from the two preceding objections that the proposed scheme is likely to result in a diminution of the provisions at Nature's feast, to use a Malthusian metaphor. It is now to be added that the number of guests will probably be increased. There will be a serious stimulus to population. Now the pressure of population on resources may not be very alarming in this country at present. But it is tenable that as regards this danger we are only enjoying a reprieve, 'an age of economic grace' (cp. Marshall, *Economic Journal*, 1907, p. 10). Is it wise to commit the country to a system which may prove unsuitable, yet unalterable?

v. The increase of population might be welcomed if it consisted of the higher types. But in the current proposals one sees no security for the improvement of the race. It is not suggested that Governments might use for this purpose the power which they will acquire as distributors of a bounty. Rather it is to be apprehended that the least desirable classes, say Charles Booth's Class A and Class B, will be encouraged to increase and multiply. It is argued, indeed, that the better class of artisans will be encouraged to keep up their good stock; while the undesirable class are already so improvident that no stimulus could add to their recklessness. But these arguments, based on a calculation of motives, seem precarious in view of the enormous risk involved. There are degrees of improvidence; there must be many who are not so improvident but that they may be made more so by encouragement. The endowment of parents in these classes at the expense of the income-tax-paying classes may realise the gloomiest anticipations of Dean Inge. The effect will be 'to penalise and sterilise those who pay the doles'; to precipitate the ruin of the great middle class, to which England owes so much (*Edinburgh Review*, April 1919).

21. Let us now consider some alternative arrangements which make for the advantages and avoid the dangers which have been described.

Some arrangements calculated to render the freedom of competition more acceptable follow automatically from that liberation; for the removal of restrictions on the work of women is calculated to increase their efficiency, and an increase in their efficiency will be attended, *ceteris paribus*, with an increase in their contributions to their families.

i. The burden of the family borne by its head does not increase in proportion to the number of children; for some contribution towards family expenses is often made by the elder children. It appears from an investigation recently made by Professor Bowley that in rather more than a third of the households which he examined there were 'earning children.' It is presumable that they contributed something over and above their keep to the maintenance of the family (cp. Bowley, 'Livelihood and Poverty,' p. 31). The family would be losers pecuniarily by the removal of these children. Many of these members would be daughters, by hypothesis in the future more efficient than at present.

ii. Where the number of the children is small, may not some contribution often be expected from the wife? 'It is possible to foresee piece-work arrangements to suit women who cannot work too many hours' a high authority, Mrs. Pember Reeves, observes. It may be hoped that in the future the only alternatives open to married working women will not be a whole day's work away from home, or work in a home made intolerable by the conditions of home work (as strikingly described by Mr. and Mrs. Webb, for instance, in 'Industrial Democracy,' p. 541). Something better may be expected from the progress both of physical and of economic science. Leroy-Beaulieu, who is sanguine as to this resource, characteristically hopes much from science and nothing from legislation.

iii. Leroy-Beaulieu also hopes for the contribution to a prospective family made by spinsters who expect to be married. 'The girl accumulating a *dot* by work in the factory, in order to remain at home as a married woman and bring up her family in comfort (*dans de bonnes conditions*)—this is the only real and practicable progress' ('La femme ouvrière . . . ,' p. 425). Mr. Cadbury's observations on the ways of the factory girl do not encourage us to hope much from this resource in this country at present. 'Only in very few cases are they [savings] accumulated in readiness for a marriage outfit' ('Women's Work and Wages,' p. 244 and context). But we may suppose an improvement in economic character as well as conditions.

iv. A more obvious compensation to men for the loss of wages—not, like the preceding, indirectly resulting from the circumstance which occasions that loss—would be afforded by an extension of the allowance now made in furtherance of education. They should be in *kind*; conforming to Mill's principle that what Government may provide with most propriety is the commodities which people would not have spontaneously demanded ('Pol. Econ.,' v., xi., 8).

These compensations may suffice to meet the male objection to removing restrictions on female competition.

For the further object of equalising the application of resources to the nurture of children within each grade a further extension of the last-named allowances (21, iv.) may be risked. But they should be guarded against the dangers objected to the endowment scheme (20, ii., iii. and iv.). Are those dangers sufficiently guarded against by Miss M. E. Bulkley when, in a work prefaced approvingly by Mr. R. H. Tawney, she recommends the provision of a free meal for all schoolchildren ('Feeding of Schoolchildren,' pp. 223-6)? The cost would be 12,500,000*l*. a year. That is for one meal, dinner. But of course breakfast would often be required (p. 228).

v. A plan for equalising the burden of dependent children would be especially serviceable in the case when the family is larger than the average. That case might be met by the comparatively modest subsidy proposed by Mr. Seebohm Rowntree ('Human Needs'). He estimates that the allowance necessary to secure physical efficiency 'in case of more than three dependent children' would come to only 8,000,000*l*. (if only families with incomes below a certain figure are to be subsidised).

Here may be the place to observe that Mr. Rowntree's proposal to treat widows with dependent children more generously than at present is not nearly so open to the objections above enumerated as the endowment of motherhood in general.

vi. Some further suggestions may be obtained from the schemes now under consideration in Australia. It is proposed to levy on every employer a tax of so much per employee, and from the proceeds to form a fund which is to be distributed among mothers according to the size (perhaps also the needs) of the family. The proposal—like that of the Endowment Committee—probably owes its chance of being accepted partly to the belief that the cost of the plan will not fall on those who are benefited by the plan, but on the employer, or the capitalist, or that supposed independent and abundant resource, the State.

But if equality of provision for children within each class is sincerely desired—without the *arrière pensée* of equalising the incomes of different classes—a simpler plan is suggested. It is open to any association of men—a trade union, for example—to resolve that each member of the association should contribute a quota of his earnings towards the formation of a fund which is to be distributed among the wives of members in accordance with the size of their families. This plan would be much less open to the objections above enumerated than the endowment of motherhood by the State. It would not disturb the labour market or the financial system. It would not require legislation. Persuasion would suffice. Those who believe that such equalisation is desirable, and that there is a chance of its being accepted, should start a campaign of argument and exhortation. Bachelors and childless husbands should be persuaded to support a fund by which they may hope one day themselves to benefit as future fathers of families.

22. To sum up; equal pay for equal work, in the sense of free competition between the sexes, has been advocated, with some reservations and adjustments. Desperate disordered competition, tending to the degradation of labour, is supposed to be excluded. There are suggested compensations to families for the loss sustained by the male breadwinner through the increased competition of women. Among such compensations the endowment of motherhood on a large scale by the State is not included. The advantages weighed are economic in a strict sense. The balance may be affected when welfare or well-being in a wider sense is taken into account.

APPENDIX.

[The references are to the sections of the text and to the paragraphs of the sections.]

1. Locke considers that the 'complexedness' of moral ideas renders demonstration difficult. But 'clearly distinct ideas,' though moral, admit of demonstration ('Human Understanding,' book iv., ch. 3, ss. 8, 19). We may extend this remark from demonstration of certainties

to deduction of probabilities (*loc. cit.*, ch. 17, s. 2); and so hope that the reasoning about welfare in the sequel may be as legitimate, if only the ideas are clearly distinct, as the more familiar purely economic reasoning proposed to the present study.

2, par. 1. The definition given by the (majority of the) War Cabinet Committee is at s. 211 of their Report [Cmd. 135], 1919. Professor Bowley's definition is in the first column of p. 177 of (Appendices to) the 'Report on Women in Industry' [Cmd. 167], 1919. Mrs. Fawcett adopts Miss Eleanor Rathbone's definition, which is substantially identical with our first definition, the one proper to the present study (*Economic Journal*, 1918, p. 3). It is quoted in part below (14).

2, par. 3. The isolation professed by Jevons is perhaps more easily maintained in dealing with Exchange and Interest than when with respect to Population. When considering the future of a population as affected by different economic conditions it is natural to have in mind a quantity of the kind which Burke, referring to the population of America, described as 'so large a mass of the interests and feelings of the human race.' I follow, however, classic precedent when I include in the present study, though purporting to be purely economic, some reference to the effect of proposed measures on the growth and condition of the population (below, 20, iv., *et passim*).

2, par. 3. It may well be that the 'complexedness' (above, note to s. 1) of the objective aggregate, the national income (as to which see Bowley, *Economic Journal*, March 1922), may exceed that of the subjective aggregate, which will constitute the central conception of the sequel dealing with welfare.

4. As to the relation between maximum and greatest possible advantage, see Pigou, 'Economics of Welfare,' Part II., ch. ii., s. 7 *et seq.*, restating the doctrine of his 'Wealth and Welfare,' which has been paraphrased by the present writer in the *Economic Journal* for June 1913, p. 215.

4 (*bis*). As to the effect of subsidies see Pigou, 'Economics of Welfare,' Part V., ch. vii., s. 3, restating 'Wealth and Welfare,' Part III., ch. viii., s. 3. *A priori*, if we construct Supply-and-Demand curves of the ordinary type, the abscissa representing the quantity of work offered, and the ordinate the corresponding rate of wage, different cases are presented. If the Supply curve consists of a horizontal line at a certain height, corresponding to the classical conception that any amount of labour will be forthcoming at the cost fixed by the labourer's necessaries, then the effect of a small or moderate bounty will be to lower wages by the full amount of the supplement. A case like this is supposed in sections 4 and 20. But the effect of a considerable subsidy may well be to alter the balance between work and leisure, to change the shape of the Supply curve, raising it and rendering it inelastic in such wise as to make the market better for competitors that are not subsidised. Examples of both results, according to circumstances, will be found in the evidence given by Miss Collett and others before the Commission on Home Work, 1907. No. 86. It will not be irrelevant to quote Miss Collett's judgment about the effect of a large subsidy. 'The girl who earns 100*l*. a year by her work and receives

another 100*l*. a year in one form or another from her father is in all probability not underselling anyone, but may, even by her liberal views of what is a good salary, be inciting her less luxurious colleagues to raise their standard of living and remuneration' (*Economic Journal*, 1898, p. 544).

4 (*ter*). On the all-important question where to draw the line which separates the genuine labour market from those whom it is desired to rule out as spoiling the market, I cannot pretend to an important opinion. It may be granted that strong measures and exceptional treatment are required for the 'residuum of persons who are physically, mentally, or morally incapable of doing a good day's work with which to earn a good day's wage' (Marshall, 'Principles,' p. 714, ch. 5). But it is often proposed to fix a minimum at a higher point; so high as to exclude great numbers. Consider, for example, a case suggested by Professor Taussig ('Minimum Wage for Women,' *Quarterly Journal of Economics*, 1915). Suppose that a large proportion (we need not suppose with Taussig a large majority) of the women in a community living at home, and so realising the advantages of co-operation, 'the economy of family life,' do well for themselves and their families by earning \$6 a week. They are not to be ruled out as 'parasitic'; given that the family would be worse off if an earning daughter were removed (*loc. cit.*, pp. 418-419). Now suppose that the 'necessary cost of proper living' for a woman dependent on herself is authoritatively determined to be \$8 a week. Would it be 'immoral' on the part of the home-residents to take less than \$8, as might perhaps be inferred from some authoritative dicta? (Cp. *Economic Journal*, 1915, p. 627, and 'What we want and why,' 1922, p. 245.) What is to be done about independent women workers in such a labour market? Shall we adjust the minimum wage to the family rather than the individual? (Cp. Marshall, *loc. cit.*, especially note to p. 419.) I do not feel able to add to what Professor Taussig has wisely written on the subject.

It should be recalled that throughout this inquiry we are abstracting humanitarian considerations; on classical lines we are seeking purely economic advantages, including the production of an efficient progeny.

5. The advantages theoretically obtainable by the scientific protection of infant industries are well exhibited by Professor H. O. Meredith (*Economic Journal*, 1906). But he adds: 'I know no case in which Protection has demonstrably done more good than harm.'

6. The property of a maximum referred to as relevant to the present study and the sequel is thus stated by Dr. Marshall: 'When the adjustment is such as to give the best results a slight change in the proportions in which they [resources] are applied diminishes the efficiency of that adjustment by a quantity which is very small relatively to that change—in technical language, it is of "the second order of smalls"' ('Principles of Economics,' p. 409, note, edition 5). Mr. Bickerdike has made an interesting application of the property to the theory of Free Trade (*Economic Journal*, Dec. 1906). His argument is discussed by the present writer in the *Economic Journal* for September 1908. As a simple illustration of the property under consideration there is adduced the incident that in the neighbourhood of

the summer solstice during eight days there is no change at all in the length of the day as given in 'Whitaker's Almanack'; whereas in other months there is a difference of two or three minutes from day to day. A more exact illustration may be obtained from the 'Nautical Almanac.' There the average hourly change in the distance of the sun from the celestial equator (due to his movement on the celestial sphere along the ecliptic) is given from day to day. For the year 1923, on June 22 the variation of the ('apparent') Declination in an hour (at noon) is given (in the volume published in 1920) as .04 of a second (angular measure), whereas on March 19 the corresponding hourly change was 59.29 seconds. Thus, considering the distance of the sun from the equator (the North Declination) as the dependent magnitude, dependent on the time, we find that the dependent magnitude when in the neighbourhood of its maximum varies per unit of the independent variable by an amount which is 1,482 times less than the variation at another date. The contrast is less striking, but still marked, if we observe a neighbourhood less close to the maximum and do not select the most favourable date for the purpose of comparison. The change in the neighbourhood of a maximum (or minimum) would often be thirty times less than the change at a distance. Similar contrasts are presented by the Declination of the Moon; for which the average change per period of ten minutes is calculated for every hour. With less precision than in physical science (it is not ours to calculate what will happen in every ten minutes of 1923), this characteristic property of a maximum is fulfilled in economics.

An illustration from the theory of monopoly is given in the *Economic Journal*, 1908, p. 401, where the law of demand is supposed (after Cournot) to be

$$y = a / (400 + p^2).$$

9. Among the many who, following J. S. Mill (*Pol. Econ.* II. xiv, 5), have noticed the crowding of women into comparatively few occupations may be specially mentioned Mr. J. H. Jones, who contributes the supplementary proposition that these occupations (before the War) 'resembled each other far more closely than they resembled the avenues which were closed to them (women) by rule or custom' ('Report on Women in Industry' [Cmd. 167], 1919, p. 182, col. 2, par. 2). Query whether much importance now attaches to the supplementary proposition submitted by Leroy-Beaulieu ('Le travail des femmes . . .' p. 136) that the specially feminine occupations do not admit of much division of labour or frequent intervention of mechanism?

10. The grounds of the assertion that the equilibrium of the labour market is apt to be, even theoretically, somewhat indeterminate are to be sought in the writer's essay on 'Mathematical Psychics.' We may begin by considering two extreme abstract cases: (1) a market consisting of an equal number of masters and men; subject to the conditions (α) that no man can (or at least does) serve two masters simultaneously, (β) no master employs more than one man; (2) a market in which the number of masters (though absolutely large, and so favourable to competition) is small relatively to the number of men; subject to con-

dition α, but not to condition β. In the first case equilibrium is thoroughly indeterminate (*op. cit.* p. 46). In the second case the employers competing against each other can beat down profits to a determinate point (the point $\eta\xi$ in fig. 1 on p. 28, the point T in fig. 5 on p. 114 *op. cit.*). But when a start is made at a point more favourable than that to the men, the competition of the men against each other (owing to condition α) being imperfect, there does not exist a determinate position towards which the higgling of the market will tend. It may be presumed that this conclusion remains true of the concrete labour market in which condition α is almost universally fulfilled.

10, par. 2. On the utilitarian principle of distribution, in the absence of perfect competition, I may refer to what I have said in the *Economic Journal*, 1897, p. 552, and to my lecture on 'The Relations of Political Economy to War,' p. 15 *et seq.*

11. Plato hardly commits himself ('Republic,' 455D) to the statement too roundly attributed to him by Grote 'that women were inferior to men in weaving no less than in other things.' But no doubt he considered them to be generally less efficient: ἐπὶ πᾶσι δὲ ἀσθενέστερον γυνὴ ἀνδρός.

11 (*bis*). Professor Cannan, in his important contribution to our subject ('Wealth,' p. 202 *et seq.*), realises the difficulty of comparing the earnings of a children's nurse with those of her brother in his occupation of, say, carting coal. Professor Cannan appears to regard as possible what I have described as probable, that even if all restrictions on entry into occupations and on education were removed, the field within which women show themselves superior to men would continue to be smaller than that in which men show themselves superior to women (*loc. cit.* p. 205—the smaller capacity implied in this statement may be 'in part the explanation' of the present lower wages of women).

12. With respect to the presumption that, even if all restrictions were removed, the (time-) earnings of women would normally be less than those of men, some specific evidence is forthcoming in the case of the cotton-weaving industry—a strong case if women are particularly well qualified for that work. Yet even in that industry, 'though the earnings are computed on the same table of piece-work prices, the men average more per week than the women' (Mrs. Sidney Webb, *New Statesman*, August 1914, p. 525). This statement is borne out by the 'Report on Earnings and Hours' [Cmd. 4545], 1906, where the average weekly earnings for men are stated to be 29*s.*, for women 18*s.* 8*d.* (p. xxxv).

14. With reference to 'secondary' differences between the sexes, Mr. J. H. Jones's observations on the 'potentiality' of the worker ('Report on Women in Industry' [Cmd. 167], p. 185, col. 1) are instructive. They show that 'equal pay [in our sense] would mean to the employer a higher piece-rate for the man than the woman for precisely the same job . . . economic value is not always fully represented in the actual product from week to week.'

14 (*bis*). With respect to the two systems whereby secondary differences can be allowed for, either two separate rates for men and

women (ι and ι'), or one common rate (ι''), Prof. Henry Clay well says: 'While the second system (of which the one first named here may be considered a species) is theoretically more economical, I do not think it is practicable. Except where a district list of straight piece-rates is possible, varying earnings are a cause of discontent and friction. Varying rates, whether time or piece, are difficult to establish, because productivity is difficult to measure. . . . A single rate . . . is the only safe policy to pursue'; 'it is probably cheaper to face the disadvantages of the single-rate system' ([Cmd. 167], p. 178, col. 2). Cp. Mrs. Sidney Webb [Cmd. 135], p. 280.

14 (*ter*). The student may be advised to illustrate the working of the different systems diagrammatically. The following hints may be serviceable. Represent the number of employees in an occupation in which only men or only women are employed by a segment of the abscissa, and draw an ordinate representing the normal weekly wage for an individual in that occupation. In the case of an occupation in which both sexes are employed the segment may be divided into two parts; the right representing the number of men, the left the number of women. The ordinate on the right would generally be higher than the one on the left. The *rates* might be represented by perpendiculars to the abscissa measured downwards. The effect of substituting for the rate which would be best in the absence of secondary differences, either a system of two rates, 1 and $1'$, or a single rate $1''$ (<1), would be made visible.

15, par. 1. On the payment of school-teachers, Mrs. Sidney Webb, in the course of her interesting articles on the right of the woman to free entry into all occupations, in the *New Statesman* (July-August 1914), states that 'educationists think there are already too few men on the teaching staff.' In this connection it is well said by Mrs. Webb: 'Sex, like youth or middle age, is a peculiar characteristic which sometimes qualifies and sometimes disqualifies persons for particular tasks.'

19, par. 1. It is suggested that the proposed allowances would not abolish the difference of mentality which is thus well expressed by Professor Henry Clay: 'Men's wages are higher than women's, because, having as a rule dependants to maintain, men will stand out for higher wages; the social custom so established imposes itself on the considerable minority of men who have no dependants.' 'Women, having in the majority of cases only themselves to support, will not stand out for a family wage; a social custom is established in this case also' ('Report on Women in Industry' [Cmd. 167], p. 179, col. 1). This force of habit, surviving after the cause which engendered it may have been removed, no doubt weakens—though it does not, I think, remove—the objection stated below under the head iii.

19, par. 1 (*bis*). See Pigou, 'Wealth and Welfare,' pp. 88-89, 321 *et seq*; and 'Economics of Welfare,' Book V., ch. iii., s. 8—where reference is made to the present writer's statement of the proposition as a postulate implied in the theory of free trade.

21, pars. 5 and 6. The objection numbered iv. would not be applicable to Professor Karl Pearson's scheme of endowment, according to

which 'Births beyond the sanctioned number would receive no recognition' ('Free Thought,' p. 444). Nor would objection v. be valid against Professor W. McDougall's proposal to differentiate in favour of the better breeds' ('National Decay').

21, par. 11. The origin and features of the Australian plans for the endowment of children are described in the *Economic Journal*, 1921, by Professor Heaton. (See also Miss Eleanor Rathbone's description of the South Australian scheme in that Journal, 1922.) Justice Higgins's classical decision in 1907 had defined as a 'fair and reasonable' minimum or 'living' wage one which provided for 'the normal needs of the average employee regarded as a human being living in a civilised community'; these needs including the support of a wife and three children. In 1920 it was found (by the Royal Commission on the Cost of Living), partly by taking account of the rise in prices, partly through the use of statistics not available to Justice Higgins in 1907, that to carry out his ideal of a living wage there would be required a much higher figure than he had fixed, no less than 5*l*. 16*s*. weekly for the average adult male worker. Now, as pointed out by the Chairman of the Commission, Mr. Piddington, there being in Australia (in round numbers) a million workers, the resources of the country would not be sufficient to furnish an average wage of this amount. But he added that so high a wage would not be required in order to make provision for children. Say there were 900,000 dependant children in all, and that 12*s*. a week was an adequate provision for each child. By giving the average workman enough to support three children provision would be made for supporting three million children—above two million 'phantom children' in addition to the actual numbers. Accordingly he proposes to fix 4*l*. as the basic wage sufficient to support man and wife without children, and to make provision for the children by a tax of so much per employee, payable by the employer.

Suppose now (as in the text above) that the children's fund is raised by (associations of) the workers themselves. The Australian statistics enable us to form an idea of the quota which would be required from each member. On Mr. Piddington's reckoning it was thought reasonable that, while a million times 4*l*. per week accrued to the workers, 900,000 × 12*s*. should be provided for the children. Supposing, then, that the whole of the provision came directly out of the workers' hands (4*l*. remaining for the use of man and wife), this would mean that on an average the worker contributed to the children's fund 10.8*s*. out of a wage of 90.8*s*., that is, about 11 per cent. The proportion might be applied to actual earnings, as distinguished from a legal minimum, in other countries (with correction if necessary for a difference in the proportion of young children to adult men). The provision for children would not be so generous as that demanded by the Endowment Committee. But it would avoid some of the objections to their scheme.

RAILWAY PROBLEMS OF AUSTRALIA.

ADDRESS TO SECTION G (ENGINEERING) BY

PROFESSOR T. HUDSON BEARE, B.A., B.Sc., D.L.,

PRESIDENT OF THE SECTION.

In 1914 the Association held its annual meeting in Australia, and, thanks to the excellent arrangements made by the Local Committees and the Commonwealth and State Governments, the visiting members had exceptional facilities for travelling over extensive areas of the great island continent. The visitors on arriving in Western Australia found that they were still cut off from the rest of Australia by a sea journey of 1,025 miles, the East-West Transcontinental Railway being then still uncompleted; they also found later on that the railway journeys from Melbourne to Sydney and from Sydney to Brisbane involved in each case a change of carriage at an intermediate station, owing to break of gauge; they thus had practical experience of the need of the solution of the two railway problems I propose to discuss in this address.

I began my professional career in the service of the Public Works Department of South Australia, at that time engaged on an important scheme of railway development in the rich wheat-growing northern areas of that province, under what was called the Boucaut policy, and I have since that date followed closely the development of the railway systems of Australia, as in the methods of construction and working there were important differences from railway practice in this country. While in Australia in 1914 I specially devoted my time to a study of the two great problems which were then and are still engaging the attention of the people of the Commonwealth—namely, (1) the unification of the existing railway gauges, which are such a serious handicap to railway transportation between the various States, and (2) the opening up of the tropical areas of North Australia by a system of railways linking up with the existing railway systems of the southern and eastern areas of the continent.

The first is a problem which in great measure affects only the people of Australia, though, as will be shown later, its solution involves the expenditure of such a large sum of money—much of which will go in the purchase of permanent way material and rolling-stock—that the manner in which it is solved must be a matter of considerable interest to the iron and steel industries of this country. The second question is one which I venture to suggest is of paramount importance to the whole of the Empire. The future safety of Australia depends upon securing such a rapid increase in the rate of the growth of its population that any idea of a hostile attack upon it would become a hopeless proposition. The vast empty spaces of Central and North Australia are a temptation to the rapidly increasing races of certain Asiatic

countries, which must be removed if the white Australian policy is to be maintained, as I personally hope it will be. It is impossible to expect any satisfactory development of the natural resources of the tropical areas of Australia, and the much-needed closer settlement, until there are safe and speedy means of communication between this part of Australia and the southern and eastern areas; this can only be provided by railways.

Unification of Gauge.

When railway construction was started about 1854—practically simultaneously in New South Wales and Victoria—it was intended to adopt the 5 ft. 3 in. gauge in both States, but a change of professional adviser at Sydney brought about a change of view, and the New South Wales Government decided to adopt the standard 4 ft. 8½ in. gauge, and this gauge has been maintained up to the present date on all the New South Wales railways. Victoria, on the other hand, adopted the 5 ft. 3 in. gauge from the first, and has, with the exception of a trifling length of 2 ft. 6 in. gauge track, built all her State lines on that gauge. South Australia, when railway construction was begun in 1855, also chose the 5 ft. 3 in. gauge and adhered to this gauge for some years; later on, however, in order to reduce the first cost of construction, a considerable length of railway mileage was constructed on the 3 ft. 6 in. gauge. Queensland and Western Australia, when they began railway construction, selected the 3 ft. 6 in. gauge because of the possibilities the narrower gauge offered in reduction of capital outlay. Briefly, therefore, four of the five mainland States have uniform gauges in their respective areas, and there are three of these gauges—5 ft. 3 in., 4 ft. 8½ in., and 3 ft. 6 in.—while the fifth State has two gauges, 5 ft. 3 in. and 3 ft. 6 in. The Commonwealth, when it was decided to build the East-West transcontinental line, chose the 4 ft. 8½ in. gauge.

As soon as the four capitals—Adelaide, Melbourne, Sydney, and Brisbane—were connected by railways the delays and additional working expenses inevitable with any change of gauge began to be realised, and as early as 1888 Mr. Eddy, Chief Commissioner for the New South Wales Railways, drew attention to the matter and urged that steps should be taken to secure unification of gauges. In 1897 a Premiers' conference, held in Adelaide, instructed the various State railway commissioners to report on the whole question of the unification of gauges. A conference was therefore held in Melbourne in August 1897, and this conference unanimously agreed to recommend that 4 ft. 8½ in. be adopted as the standard Australian gauge; the decision was based on the fact that it would be cheaper to convert the 5 ft. 3 in. to 4 ft. 8½ in., and that 3 ft. 6 in. was too narrow a gauge for main-line traffic. It was estimated that the cost of unification would be about 23,600,000*l*. None of the States took any steps to carry out the recommendations of the conference.

In 1903 it became necessary to decide upon the gauge which should be adopted for the East-West transcontinental line to connect Western Australia with the eastern provinces, the length of the line being about

1,063 miles. A conference of the Engineers-in-Chief of the five States unanimously recommended that 4 ft. 8½ in. should be the gauge of this important line, and eventually the line was built on this gauge. This decision was later on confirmed at a meeting in 1911 of the Railway War Council, attended also by the Chief Commissioners of the various State Railways.

Mr. Hales, a well-known engineer of Launceston, Tasmania, in 1911 urged the Commonwealth Government to reconsider the question as to which gauge should be adopted as the standard for the whole of Australia: he was of opinion that the 5 ft. 3 in. gauge would enable locomotives of 20 per cent. more power to be used as compared with the 4 ft. 8½ in., and 10 per cent. more traffic could be carried on the same mileage, and that it would be easier to secure a market for discarded 4 ft. 8½ in. stock. Mr. H. Deane, the Chief Engineer of Railways to the Commonwealth, was, as a result of Mr. Hales' memorandum, instructed to reconsider the whole question of the standard gauge and to report as to whether it would be advisable to reverse the earlier decisions and to select the 5 ft. 3 in. gauge. Mr. Deane in his report advised that the decision to adopt the 4 ft. 8½ in. gauge should be adhered to; he was opposed to the adoption of any narrower gauge than that, and he pointed out that a 5-chain curve could be used even on a 4 ft. 8½ in. gauge track under certain conditions, and that such lines had been constructed not only in Australia but in other countries. On the other hand, the speed limit for a 3 ft. 6 in. line was far below that which could be safely adopted on a 4 ft. 8½ in. line, even on a straight track in flat country. In concluding his report Mr. Deane urged the need of a prompt settlement of the matter, as each of the States was steadily increasing its railway mileage and every year unification was postponed meant that the cost would be steadily mounting up. Unification of the gauges would facilitate the transfer of rolling-stock from one State to another during any emergency, and considerable economies in running expenses would be obtained by better utilisation of existing stock; for example, a train from Sydney, reaching Albury (the change-of-gauge station) in the early morning, was compelled under existing conditions to remain idle in that station all day before returning to Sydney.

With regard to the method of conversion, Mr. Deane advocated the third-rail method, and he gave in his report a brief account of the various methods which had been adopted in other countries where unification of gauges had been carried out. In the United States, where the rolling-stock is always of the bogie type, the method adopted had been to remove the bogies of one gauge and substitute those of the other—quite a cheap and convenient method, but obviously impossible with four-wheeled stock. Various methods had been suggested for dealing with the problem of four-wheeled stock, such as the use of divided axles with inside bearings or a sliding axle, but there were serious practical difficulties in both methods, and both methods introduced a sense of insecurity. Another method, which had been proposed by Mr. Bolton, was to fit a third wheel just inside one of the wheels of the broader gauge stock; again, there were substantial practical difficulties—the third wheel must be kept always on one side of the track, without

turning the vehicle, and it would be difficult to carry this out at junctions where branch lines came in from both the right and the left. Mr. Deane's own idea was that the third rail producing a mixed gauge would be the best solution of the problem. This would involve difficulties at points and crossings. Mr. Brennan, the well-known torpedo expert, had prepared a design for compound switches, and a model of this had been made at the Sydney Railway Interlocking Workshops. Mr. Deane was of opinion that this design of Mr. Brennan would probably overcome the difficulties introduced by the use of the third rail, and he suggested to the Commonwealth Government that two or three full-size sets of the switches should be constructed and tried at an important junction station. Mr. Deane was of opinion that the change-over as regards Victoria and South Australia, if carried out on these lines, would occupy from five to ten years. The table which follows gives the lengths of the various gauges, the original capital cost, and the cost per mile of the State Railways on June 30, 1910, that is at the date when Mr. Deane was preparing this report.

Mileage and Gauge of Australian Railways on June 30, 1910.

State	Gauge			Capital Cost	Cost per Mile
	5 ft. 3 in.	4 ft. 8½ in.	3 ft. 6 in.		
				£	£
New South Wales	—	3,643	—	48,925,348	13,430
Victoria . .	3,383	—	—	42,453,801	12,549
South Australia .	599	—	—	6,670,794	11,136
South Australia .	—	—	1,313¼	7,737,738	5,892
Northern Territory	—	—	145½	1,180,155	8,111
Queensland .	—	—	3,661	24,336,372	6,648
Western Australia	—	—	2,144½	11,377,062	5,305
Tasmania . .	—	—	445¾	3,949,441	8,860
Total . .	3,982	3,643	7,710	146,630,711	—

In 1913 a further conference of the Engineers-in-Chief was held, and this conference adopted two resolutions of considerable importance:

1. That it was advantageous that the work of unification should be undertaken at once, since the longer the work was delayed the greater would be the cost.

2. That the relative advantages of the 5 ft. 3 in. and the 4 ft. 8½ in. gauges from the point of view of efficiency and economy of working, and discarding the question of interest on cost of conversion, approximately balanced one another, and that, since the cost of conversion of the wider to the narrower gauge was much less than for the converse operation, they recommended the adoption of the 4 ft. 8½ in. gauge.

This conference estimated that the cost of converting all the railway lines of Australia to the 4 ft. 8½ in. gauge would be 37,164,000*l.*, but, if it were decided merely to unify the main-line routes connecting the various capitals, the cost would be 12,142,000*l.* Of this latter sum the new lines which would be required would account for 4,847,000*l.*, and the conversion of the existing 5 ft. 3 in lines (all the Victorian but only some of the South Australian) would cost 7,295,000*l.*

This conference emphasised the need of an early solution of the problem by drawing attention to the fact that, while the cost of converting the New South Wales 4 ft. $8\frac{1}{2}$ in. lines to 5 ft. 3 in. had been estimated in 1897 at about 4,250,000*l.*, the conference now estimated that this work would cost 19,250,000*l.*; and, similarly, the cost of converting the Victorian and South Australian 5 ft. 3 in. lines to 4 ft. $8\frac{1}{2}$ in. had been estimated in 1897 at 2,250,000*l.*, and the new estimate was 7,250,000*l.* To account for this greatly increased cost it was pointed out that in the sixteen years which had elapsed between these two estimates being prepared the railway mileage in New South Wales had nearly doubled, traffic and rolling-stock had greatly increased, and the cost of wages and materials had gone up from between 50 per cent. to 150 per cent.

The decision of the Commonwealth Government to adopt the 4 ft. $8\frac{1}{2}$ in. gauge for the East-West transcontinental line, and the construction of that line on that gauge, ought to have settled definitely the choice of the standard railway gauge for Australia, but the question was re-opened, and a Royal Commission was appointed on February 8, 1921, to report on the whole question of the standard gauge which should be adopted for Australian railways, and to submit estimates of the cost of conversion and recommendations as to how the work should be carried out. This Royal Commission unanimously recommended that the previous decisions as to the adoption of the 4 ft. $8\frac{1}{2}$ in. gauge should be adhered to; they were of opinion that no important gain in the carrying capacity of the railways would be secured by using the wider 5 ft. 3 in. gauge, while the reduction in the cost of conversion would be considerable if the 4 ft. $8\frac{1}{2}$ in. gauge were adhered to.

Much confusion has arisen in discussing the gauge question by the failure on the part of many of those who took part in the controversy to appreciate the difference between 'track gauge' and 'load or structure gauge.' At the present time locomotives are in use on 4 ft. $8\frac{1}{2}$ in. gauge lines giving a static pressure of 35,000 lb. between the rail head and the wheel tread, and such a pressure produces probably the maximum permissible deformation in the metal of the rail head and the wheel tread, hence it is the quality of the metal used in the rails and in the tyres which determines ultimately the carrying capacity of the 4 ft. $8\frac{1}{2}$ in. or any other gauge. On the other hand, the structure gauge determines the density the load must have in order to load the wheels to their maximum capacity; and it is, therefore, to structural gauge changes that attention should in the first place be given. The Australian 1905 uniform structure gauge, when outside cylinder locomotives are used, permits, as a matter of fact, the use of bigger diameter engine cylinders on a 4 ft. $8\frac{1}{2}$ in. gauge track than on a 5 ft. 3 in. gauge line, as shown in the lantern plate. On the other hand, the 5 ft. 3 in. gauge permits a higher centre of gravity with the same stability and ease of riding, but this higher centre of gravity is unobtainable with the usual goods traffic on Australian lines. Undoubtedly it would cost less to change from the wider to the narrower gauge than to carry out the converse operation, since in the former the same sleepers can be used, and no changes in banks, cuttings, and ballast are required, and in the conversion of the rolling-stock the change

from the wider to the narrower means shortening the axles of the rolling-stock wheels, a simple matter, while to carry out the reverse operation of lengthening these axles would be practically impossible. At the present time there are about 60,000,000 sleepers on the Australian railway lines, of which about half are on the 3 ft. 6 in. gauge lines. The average life of a sleeper in Australia is about twenty years, and, therefore, the annual renewals run to about 3,000,000 sleepers, but, owing to the results of war conditions, the annual renewals at the present time are nearly 5,000,000. Some 75 per cent. of the 3 ft. 6 in. sleepers now in use are 7 ft. long, and such a sleeper could be used with a 4 ft. 8½ in. gauge if for each rail four new 8-ft. long sleepers were introduced along with old 7-ft. sleepers, one at each end and the other two equally spaced in between, provided that only 60-lb. rails were used and that the traffic was neither heavy nor fast. If such an arrangement were adopted there would be a saving of something like 12,500,000 sleepers during the process of conversion of the gauges.

The Commission considered very carefully the various proposals which had been made to obviate the need of the conversion of the running track, such as, for example, the third-rail method and the many mechanical devices which had been suggested for allowing the same rolling-stock to be run over different gauges, and unanimously turned them all down; in fact, a special board of experts had been appointed in 1918 to examine and report upon a number of these mechanical devices and suggestions, and had been unable to report favourably on any of them. All such devices were merely in the nature of temporary plans for postponing the ultimate conversion to a uniform gauge; they therefore involved additional expenditure and an increase in the final total cost of conversion. The Commission recommended that the unification should be carried out gradually by shifting one of the two rails of the 5 ft. 3 in. gauge inwards, and shifting both the rails in the case of the 3 ft. 6 in. gauge outwards, the work to be done in stages and temporary change stations to be arranged for, the traffic being diverted as far as necessary while the length of track between two change stations was being altered.

This Royal Commission went very fully into the cost of the work of conversion; first, for the provision of a main line only on one gauge from Fremantle to Brisbane, leaving all other State lines unconverted; and, secondly, from the point of view of bringing the whole railway system of Australia to one uniform 4 ft. 8½ in. gauge, and independent estimates were prepared for each scheme.

Cost for Converting the Main Line only, from Fremantle to Brisbane, to a Uniform 4 ft. 8½ in. Gauge.

Three alternative routes were proposed (as shown in the lantern plates):

Route	Cost £	Length of Track. Miles
A	17,850,000	3,356
B	19,583,000	3,243
Modified A	18,579,000	3,356

It may be pointed out that the present mileage from Fremantle to Brisbane is 3,448 miles, and that the chief reduction in the mileage would be brought about by adopting a coastal route from Sydney to Brisbane instead of the present route, shortening the distance between these capitals from 715 to 616 miles. If the main-line route alone were provided for, serious complications and a great increase in the cost of operating the unconverted 5 ft. 3 in. lines in Victoria and South Australia would ensue, and the Commission therefore were of opinion that the other 5 ft. 3 in. lines in both States would have to be converted at once to the 4 ft. 8½ in. gauge, bringing up the total cost to somewhere about 21,600,000*l.*

Conversion of all Lines to the 4 ft. 8½ in. Gauge.

The Royal Commission estimated this would involve a capital expenditure of about 57,200,000*l.*; this estimate made provision for the necessary transfer temporary stations as well as the actual work of conversion, but did not provide anything for the cost of transfer of goods and passengers during the transition period, or for interest on capital expenditure while the work was being carried out.

The Commission recommended the appointment of a director to carry out the whole work, who should be assisted by a competent professional staff. In forwarding their report to the Commonwealth Government the Chairman raised the important question as to whether the huge expenditure which would be required would be justified under existing conditions of the money market and the present high cost of all engineering works.

Method of Changing the Track from the 5 ft. 3 in. Gauge to the 4 ft. 8½ in. Gauge suggested by the Royal Commission.

On the existing 5 ft. 3 in. lines the rails are usually canted inwardly from about 1 in 20 to 1 in 26, though actually in practice the canting varies from 1 in 12 to 1 in 40, and at crossings the rails are kept flat. As far as is known at the present time there is no practical or theoretical reason why one rail of a track should not be on the flat while the other rail is on a cant. It would much facilitate the work of removing inwards one of the rails if this rail could be laid on the sleeper in its new position on the flat, the other rail being left undisturbed. It would be desirable, however, to test this question experimentally by actually altering one of the rails on a short length of existing main track from the canted to the flat position; if it were found that high-speed traffic could be safely carried on under such a condition, the experiment might be further extended by converting to this condition a length of about 50 to 60 miles; if again the running results were satisfactory, this method might be adopted throughout during the process of conversion. If this plan were adopted there would be no necessity to adze the sleepers for the new rail position, and the only operation required on the sleepers would be the boring of the holes for the dog spikes in the new position of the rail; this could be done throughout before any attempt was made to move the rails. It would be better from the point of view of securing a symmetrical position of the rails on the sleepers to move both rails inwardly by the necessary small amount.

but this would involve boring double sets of holes, and one set of holes on each side could not be bored out until the rails were lifted from their existing position. The work could be carried out in the following order: Temporary permanent-way gangs would carry out the work of adzing (if this were necessary) and boring the sleepers for the new position of the rails, and would partly drive in most of the inside spikes for the new location of the rail, drawing at the same time many of the old inside spikes. When this work was complete the next operation would be to draw the remaining spikes of the rails to be shifted and then to push inwards the rails in long lengths; there would be no necessity to interfere with any of the fish-plates. The permanent gangs of platelayers would follow up the work of these temporary gangs and would complete the accurate gauging and spiking of the track, and at the same time they would draw any spikes left in the old position. Two constructional trains, one of the 4 ft. 8½ in. gauge following up, and one of the 5 ft. 3 in. gauge going ahead, would be needed. At all tunnels and stations it would be necessary to slew over the whole track 3½ inches in order to keep the existing track centres.

With regard to the rolling-stock, if details were carefully worked out beforehand, no serious practical difficulty would occur in changing from the 5 ft. 3 in. to the 4 ft. 8½ in. gauge, though, in the majority of the locomotives, new fire-boxes would be necessary besides the requisite alterations to the frames and axles.

Changing over from the 3 ft. 6 in. Gauge to the 4 ft. 8½ in. Gauge.

This would be a much more elaborate and difficult job, as both rails must be moved outwards 7¼ inches, and all earthworks, bridges, and tunnels widened so as to be suitable for the increased gauge and new width of formation; there would be, therefore, considerable dislocation of traffic while the work was being carried out, and it would be necessary to divide the country up into a series of areas and deal completely with all the lines in one particular area before any work was started in another area.

Cost of Conversion.

In preparing their estimates for the cost of the conversion of the main lines only, the Commissioners based their figures on the employment of an 80-lb. rail and the necessary consequent improvements in road-bed, bridges, &c., to allow for the heavier rolling-stock which would be employed if an 80-lb. rail were in use. They had also in their estimates provided for the cost of the temporary transfer stations and the new permanent stations which would be required at Adelaide (estimated cost 500,000*l*.), Melbourne (estimated cost 880,000*l*.), Brisbane (estimated cost 150,000*l*.). If, however, it were decided to convert all the 5 ft. 3 in. lines at once to the 4 ft. 8½ in. gauge, much of this costly main station expenditure would not be required. The estimate prepared by the Commissioners of the expenditure required for the work of complete conversion differs very greatly from the estimate submitted by the five State Railway authorities, and the attached table shows the enormous discrepancies between the two sets of estimates.

Estimated Cost of Converting all the Railway Lines to a Standard 4 ft. 8½ in. Gauge.

State	Estimate of Royal Commissioners	Estimate of State and Commonwealth Railway Authorities
	£	£
Commonwealth . . .	2,648,000	7.320,904
Western Australia . .	11,823,000	35 669,092
South Australia . . .	8,737,000	16,782,487
Victoria	8,324,000	14,798,522
New South Wales . .	—	—
Queensland	25,668,000	53,332,028
Gross total . .	£57,200,000	£127,903,033

The discrepancy arises largely from the fact that the State Railway authorities based their estimates upon a high-standard 4 ft. 8½ in. track with 80-lb. rails for every mile of line in the State. In Western Australia, for example, where the State authorities prepared a total estimate amounting to about 35,669,000*l.*, the estimate would be reduced to about 15,000,000*l.* if lighter earthworks and 60-lb. rails were adopted on most of the tracks, and in Queensland a similar procedure reduces the original estimate of 53,332,000*l.* to about 32,000,000*l.*, but even these modified State estimates greatly exceed the figures given by the Commissioners.

Chief Works required to give a Uniform 4 ft. 8½ in. Gauge Line suitable for fast, heavy Traffic from Fremantle to Brisbane.

As regards Western Australia, it will be necessary to lay a new line on the 4 ft. 8½ in. gauge alongside the present 3 ft. 6 in. gauge from Perth to Kalgoorlie, and to construct an entirely new bridge over the river Swan. In South Australia there is at present a very unsatisfactory length of line on the 3 ft. 6 in. gauge, with severe gradients and awkward curves, between Terowie and Port Augusta. This would be eliminated by the construction of a new 4 ft. 8½ in. line from Port Augusta to Lochiel, and by the conversion of the existing 5 ft. 3 in. line from Lochiel to Salisbury to the 4 ft. 8½ in. gauge. These two pieces of work would at once cut out two of the three present change-of-gauge stations in South Australia—viz., those at Adelaide and Port Augusta, and the Terowie change-of-gauge station would be transferred to Salisbury. The reduction in the existing heavy grades is shown by the fact that while on the present route on the 3 ft. 6 in. line there is a summit level of 2,000 ft., the summit level on the proposed new line would not exceed 400 ft. This work, if taken in hand at once, would cost about 800,000*l.*, and would in itself, without any other changes, greatly improve the present railway facilities between East and West Australia. In converting the 5 ft. 3 in. line from Adelaide to Melbourne the most important work would be a new bridge over the river Murray, suitable for the heavier rolling-stock—an expensive piece of work. In Victoria the Commissioners suggested three alternative routes, as shown in the lantern plates, but they pointed out that Route A would be very costly and difficult to work, and therefore it

would be much more satisfactory, if the remaining 5 ft. 3 in. gauge lines in Victoria were not to be converted, to adopt Route B. There is no doubt, however, that the adoption of either Route A or Route B would prove extremely unsatisfactory as regards the working of the remaining railway systems of Victoria; it would be much better to decide to convert at once the whole of the 5 ft. 3 in. Victorian lines to 4 ft. 8½ in., carrying out the work in a series of stages, as shown in the lantern plate.

Since all the New South Wales railways are on the 4 ft. 8½ in. gauge, the only works required in the State would be the completion of the coastal route northwards from West Maitland; much of the constructional work on this coastal route is already completed. When it is completed as far as Richmond Gap, and when a new 4 ft. 8½ in. gauge line is built southwards from Brisbane to join the New South Wales line at Richmond Gap, a greatly superior route will be provided between the capitals of Sydney and Brisbane. The present inland route has a maximum summit level of 4,450 ft., while the coastal route would not have a greater summit level than 800 ft.

The report of the Royal Commission was considered at a Premiers' Conference held at Melbourne in November 1921. Mr. Groom, the Federal Minister of Works and Railways, in view of the enormous cost for complete conversion, advocated that the work of providing the main-line route connecting all the capitals by a 4 ft. 8½ in. high-standard line should be undertaken at once, and also the work of the conversion of all other Victorian and South Australian 5 ft. 3 in. gauge lines to 4 ft. 8½ in. gauge. The total cost of these two pieces of work would be about 21,000,000*l*. The Premier of South Australia, however, raised serious objections, the principal one being the difficulties which would arise in the working of the local railway traffic owing to the 3 ft. 6 in. lines of that State being left unchanged, and he pointed out that his State Railway officials disagreed entirely with the estimates of the Commission in regard to the cost of the conversion of all the railway lines in South Australia to the standard gauge. They were of opinion that instead of the cost amounting to about 8,737,000*l*., as estimated by the Commissioners, it would be more like 14,750,000*l*., and, in addition to this heavy capital outlay, there would be a serious loss of revenue brought about by delays in operating the traffic during the process of conversion. He was of opinion, and his views were apparently supported by the Premier of Victoria, that the whole cost of conversion of the railways in Australia to a 4 ft. 8½ in. gauge would not be far short of 100,000,000*l*. sterling, and he thought that it would be very much wiser to spend this huge sum of money on public works which would be more quickly reproductive.

The Premiers' Conference eventually accepted the decision of the Royal Commission with regard to the adoption of the 4 ft. 8½ in. gauge, but postponed decision as to when the work should be undertaken.

The Australian Prime Minister in March last, in a public speech, drew attention to the steadily increasing cost of the work of conversion, and to the considerably increased loss in working the existing State and Commonwealth railways. For the year ending June 30, 1920, he

stated that after paying interest on loans and all working expenses there was a total deficit of 1,744,000*l*., and in 1921 this had risen to 3,946,000*l*. He expressed the view that very important economies in working expenses would be brought about by unification of gauges.

In giving this summary of the history of the break-of-gauge problem of Australia I have endeavoured to arouse interest in this country in this question. A great scheme of railway work, which is to cost anything from 50,000,000*l*. to 100,000,000*l*., and which will involve the manufacture of an enormous quantity of material, must surely be of interest to the engineers and manufacturers of this country, even if it were being carried out in a foreign country, and still more so when it is being carried out in one of our great oversea Dominions.

In spite of the decision of the Royal Commission in regard to mechanical devices for overcoming the break-of-gauge difficulties, I think the problem might still be solved by such means, though it must be admitted that none of the mechanical devices brought forward up to the present time have offered a satisfactory solution. In March last a Mr. Mathews, of Victoria, showed a model before the South Australian Railway Commissioners by which he claimed a solution of the whole problem without changes in the permanent way, except at the terminal stations where break of gauge occurred. His proposals were for certain improvements in the bogies of railway carriages and the under-carriages of trucks, so as to allow an automatic alteration from 5 ft. 3 in. to 4 ft. 8½ in. without manual labour or without power gear. Mr. Mathews claimed that a whole train could be changed to the new gauge in ten minutes, and that the only labour required for the alteration would be that of the ordinary train staff.

As I have only seen brief newspaper accounts of Mr. Mathews' proposals I can give no technical details, nor can I express any definite opinion as to the feasibility of this latest proposal. There is certainly a possibility that some mechanical device might be designed which would prove satisfactory in operation, and would postpone the need to incur at the present time the heavy charges required for complete conversion to one gauge, though undoubtedly sooner or later it is inevitable that complete conversion must be undertaken.

North-South Transcontinental Railway.

The South Australian Government, at that time in control of the Northern Territory (annexed to South Australia by Royal Letters Patent in 1863), on December 10, 1902, advertised that they were prepared to accept tenders up to May 2, 1904, for the construction on a land grant system of the 1,063 miles of railway between Oodnadatta, the northern terminus of the South Australian railway system, and Pine Creek, the southern terminus of the line from Port Darwin. The gauge was to be 3 ft. 6 in., rails not less than 60 lb. per yard, and the mileage was not to exceed 1,200 miles. This was pursuant to an Act of the South Australian Parliament passed in 1902, entitled the Transcontinental Railway Act. The lantern plate shows the proposed route.

The minimum land grant specified in the Act was 75,000 acres per mile of track; the State was prepared, therefore, to surrender about 80,000,000 acres of land as a prize for the construction of the line.

An interesting publication was issued by the State Government giving full details, as far as then known, of the nature of the country through which the line would be constructed, and of the possibilities of its future development from the agricultural, pastoral, and mining points of view. The total area of the Northern Territory was estimated at 335,116,800 acres, with a seaboard of some 1,200 miles to the Indian Ocean. The pamphlet in an appendix gave a full bibliography of the literature on Northern Australia published up to that date.

Unfortunately, owing to change of the State Ministers and to other circumstances, this scheme fell through, and no further action in the matter of the transcontinental line was taken until the control of the Northern Territory was handed over to the Commonwealth on January 1, 1911. The Commonwealth took over at the same time (1) the national debt of the Territory, largely incurred in constructing the railway line from Port Darwin to Pine Creek and other necessary works of development; (2) the 3 ft. 6 in. line from Port Augusta to Oodnadatta, the South Australian railway department continuing to work the line, but any deficit on the working and the interest on cost of construction being met by the Commonwealth Government. The Commonwealth further undertook to complete the North-South Railway under certain conditions.

The Darwin to Pine Creek railway, a 3 ft. 6 in. line, single track, with 41-lb. rails, was opened on October 1, 1889, its total length being 145½ miles; it was intended to be the first instalment of the northern portion of the transcontinental line. In 1913 the Commonwealth authorities decided to extend this line a further 54½ miles to the Katherine River, and the railway station at this river now forms the southern terminus of the line from Darwin.

In order to obtain the necessary information to enable the Commonwealth Government to implement their undertaking to complete the transcontinental railway a Royal Commission was appointed on March 28, 1913, to report upon the following matters in their relation to the development of the Northern Territory: (1) On the routes of the necessary railways and the classes of such railways; (2) the desirableness and practicability of creating new ports. The Commission, after taking evidence at Melbourne, Sydney, Adelaide, and Brisbane from the railway authorities and others interested in the development of North Australia, visited the Territory, and travelled by sea, by river, and on land some 3,000 miles; during their journeys local witnesses were examined, and the report of the Commissioners was submitted to the Commonwealth Government on February 20, 1914.

As a proof of the inaccessibility of this vast province from the rest of Australia, and of the need of railway development, I may mention that when returning to this country in 1914 after the meeting of the British Association in Australia, I left Sydney on the s.s. *Mataram* on October 1, and did not reach Darwin, the capital and seaport of the Territory, until October 15, the sea journey being 2,620 miles; from Brisbane it is 2,100 miles. In an interesting paragraph of their report the 1913 Commissioners point out that it takes longer to go by sea from the nearest State capital (Brisbane) to Darwin than it does to go from that port to Singapore or Hong-Kong. How perilous such a state of

things might be to the Commonwealth in certain contingencies needs no words from me to bring home to those who are fighting so strenuously for the white Australian policy.

Royal Commissioners' Suggested Railways.

The Commission recommended the following lines:—

1. The construction of the main North-South line from Katherine River to Oodnadatta via Renner and Alice Springs, mileage about 1,020 miles, the gauge to be 3 ft. 6 in., and work to be commenced from each end.

2. The construction of a branch line from the main line, near or at Katherine River, to serve the Victoria River pastoral area, should it be found impossible to give such a westerly swing to the main line from Katherine to Willeroo as would serve the same purpose.

3. The construction of a railway from a proposed new harbour in Pellew Island in the Gulf of Carpentaria to the Barkly Tablelands, the line following the McArthur Valley to Anthony's Lagoon.

The Commissioners further expressed the view that it would be essential for the Queensland Government to extend their railway systems to Camooweal, and for the Commonwealth to connect both the main North-South line and the Barkly Tablelands line by branch lines with Camooweal, so as to give direct railway connection between the Territory and the Eastern States of Queensland and New South Wales.

The Commissioners estimated the cost of the main transcontinental line at 5,000,000*l*. for a 3 ft. 6 in. gauge, and 7,500,000*l*. for a 4 ft. 8½ in. gauge.

The lantern plates show (1) routes of proposed lines; (2) isohyets for the Territory; (3) relation of proposed lines to existing Australian railway systems; (4) alternative railway routes suggested by Mr. Coombes in a minority report.

In 1915 the House of Representatives referred to the Parliamentary Standing Committee on Public Works a proposal to extend the Darwin Railway a further distance of about 64 miles in a south-easterly direction from Katherine River Station to Bitter Springs; this would be a further link in the transcontinental line, and would open up to profitable exploitation the newly discovered tin mines at Marranboy. The cost of construction was estimated at 320,000*l*. for a 3 ft. 6 in. line, single track, 60-lb. rails, using, however, sleepers long enough for a 4 ft. 8½ in. line, should it be decided later on to change the whole line over to this gauge; the ruling gradient was to be 1 in 100, and the sharpest curve 40 chains; the time of construction was estimated as one and a-half years.

Mr. Hobler, Commonwealth Engineer for Ways and Works, stated that the cost of the Pine Creek and Katherine River extension would be 6,000*l*. per mile, and he estimated the extension to Bitter Springs (now called Mataranka) would cost 4,938*l*. per mile. He further said it was intended to make a permanent survey of a proposed further extension to the Daly Waters telegraph station, 95 miles south from Mataranka and 360 miles from Darwin. There can be no doubt that whatever route is finally adopted for the central portion of the transcontinental

line the existing telegraph route must be followed, at any rate as far as Newcastle Waters, 90 miles south of Daly Waters and 450 miles from Darwin.

The Standing Committee, in recommending that this extension be authorised, expressed the view that it was inadvisable to use the longer sleepers, and they recommended further experiments on the possibility of using reinforced concrete sleepers on steep banks and curves. On the original Darwin Pine Creek line steel sleepers were used, and these had worn well except on the coastal section, but their use on the southern extension was impossible owing to the great increase in their cost.

The 1913 Royal Commissioners in their report had recommended a westerly swing of the main line to Willeroo to serve the Victoria River district, but the Standing Committee disapproved of this suggestion, owing to the difficult nature of the country, which would much increase the cost per mile, and would considerably increase the length of the line. A Sub-Committee of three members of the Commission inspected in July and August 1916 the whole of the country along the alternative routes, and the final finding of the Standing Committee was based on the report of this Sub-Committee.

This Committee emphasised the need of settling population on the areas already opened up by railways, not merely by taking people away from other parts of Australia, but by the introduction of European settlers. This would be facilitated by inducing railway construction men to bring out their families, and by offering land settlement facilities to them when the railway construction work was completed.

Since the report of the Royal Commissioners in 1914 a fierce controversy has been going on in the Commonwealth and South Australian Parliaments and in the public Press in regard to the North-South line, and as to the precise meaning which must be attached to the words in the agreement made between the Commonwealth and the State of South Australia when the latter ceded the Northern Territory in 1911—viz., the Commonwealth shall ' construct, or cause to be constructed, a railway line from Port Darwin southwards to a point on the northern boundary of South Australia proper,' and ' construct, or cause to be constructed, as part of the Transcontinental Railway, a line from a point on the Port Augusta Railway to connect with the other part of the Transcontinental Railway at a point on the northern boundary of South Australia proper.' The Eastern States assert that it would be a waste of national money to construct the due North and South line, as so much of the country it traverses is useless for pastoral or any other purpose, and they maintain that the line should deviate easterly from, say, Newcastle Waters into Queensland to Camooweal, and that South Australian interests would be completely met by a new line in that State, running in a north-easterly direction from Maree on the Port Augusta line to a connection with the Queensland railways near Birdsville. South Australia, on the other hand, insists that a bargain is a bargain, and that this new proposal is entirely at variance with the real meaning of the terms of the agreement. They further allege that much of the land declared worthless would be quite good country for sheep and cattle rearing if railway facilities existed and if water conservation on sound lines was carried out.

In consequence of this divergence of views as to the nature of the country through which a transcontinental line would pass, a Sectional Committee of the Commonwealth Joint Standing Committee of Public Works travelled in 1921 across the continent from Oodnadatta to Darwin by motors, explored a considerable area of country both east and west of the overland telegraph line, and examined local witnesses in order to ascertain the views of those already settled in these areas as to the most suitable routes for the proposed transcontinental lines. This Committee was accompanied by Mr. Hobler, Commonwealth Engineer for Ways and Works, who had already in 1920 travelled over the Kimberley area of West Australia, and had submitted a report on the railway lines which were required in order to open up that great cattle-rearing area, and to give that district satisfactory facilities for marketing their stock.

The Standing Committee, after receiving the report of their Sectional Committee, began to take evidence in the Southern States, and at a meeting held in Sydney last May Mr. Hobler submitted a lengthy report setting forth the conclusions he had come to in regard to the best routes not merely for a transcontinental line, but for the various other railways which were required in order to connect the undeveloped tropical areas of Australia with the southern temperate districts already fairly well provided with railway facilities. Mr. Hobler's proposals were based on the principle that the pastoral and cattle industries must be considered to be the primary ones; mining development would only, he thought, begin at a later date, and agricultural developments would only start when the primary industries were firmly established and population had begun to increase.

Two alternative transcontinental routes were suggested by Mr. Hobler, with certain essential branch lines, viz. :—

Western Route.

Oodnadatta to Emun-ga-lan	(Main line)	1,018 miles	Cost £12,077,803
Newcastle Waters to Camooweal	(Branch line)	359 miles	Cost £3,921,750
(Average cost per mile about £11,300)			
	Total mileage	1,377	Total cost . £15,999,553

This proposal would apparently satisfy the claims of South Australia, and would at the same time give a direct connection between the Eastern States and the Northern Territory.

Eastern Route.

Maree to Emun-ga-lan via Boulia, Camooweal and Daly Waters	1,320 miles : Cost £14,329,864
(Average cost per mile about £11,000)	

The lantern plates show these suggested alternative routes.

The eastern route, which was the one preferred by Mr. Hobler, would mean a saving in capital cost of 1,669,689*l*., to which would be added a further saving of 2,759,584*l*. if the widening of the existing 3 ft. 6 in. gauge line between Maree and Oodnadatta were postponed.

If this eastern route were finally adopted it would probably be necessary, in order to secure the assent of South Australia, to extend

the present 3 ft. 6 in. line from Oodnadatta to Alice Springs, 297½ miles, in order to open up for development the pastoral and mining McDonnel Range country. This line could be built at a very economical cost if permanent way, released by the widening of the existing track from Maree to Port Augusta, were utilised and practically a surface track laid, the cost working out at about 1,490,502*l.*, or 5,010*l.* per mile. Taking into consideration the cost of this line, the adoption of the eastern route would secure a saving in capital cost of 2,938,771*l.* as compared with the western route.

With regard to working expenses and receipts, the western route complete was estimated to show an annual deficit in the early years of 17,024*l.*, to which must be added interest on capital 973,927*l.*, making, therefore, an annual charge on the Commonwealth Treasury of about 1,000,000*l.* The eastern route, including the Oodnadatta to Alice Springs line, would probably have an annual excess of receipts over expenditure of 107,832*l.*, the interest on capital would be 852,638*l.*, making the annual charge on the Treasury about 744,806*l.* The eastern route would, according to these estimates, mean a saving of about 250,000*l.* sterling per annum as compared with the western route, in addition to the saving of nearly 3,000,000*l.* in the original capital outlay.

Should the eastern route be adopted Mr. Hobler anticipated a rapid development of Port Augusta. The erection of large meat works and the deepening and extension of the harbour would make this port the natural outlet for the pastoral, agricultural, and mining products of an immense area of Central Australia.

It will be seen that there is very little to choose between the two routes as regards mileage of new lines and the cost per mile. The main factor in the comparison of the two routes and the one which is most open to dispute is the expected annual charge upon the Commonwealth finance for a good number of years. Since these proposed railways are primarily development lines, they cannot possibly become a paying proposition until the expected increase of population and resultant more thorough and complete development of the great natural resources of this hitherto almost unutilised area of Australia have had time to materialise.

A very recent suggestion by the Surveyor-General of South Australia is that the transcontinental line should start from Tarcoola on the East-West line, thence run direct to Oodnadatta (a new line), from there follow the overland telegraph line to Barrows Creek, deviate then easterly, but return to the telegraph line route at Powell's Creek, and continue to follow it till it reaches the terminus of the existing line at Katherine River.

Of the two problems, the one which seems most urgent and calls for a prompt solution is the transcontinental line, with its various proposed branches. Capital can only be provided for either the unification of the gauges or for the transcontinental lines by borrowing, and it is obvious that borrowed money, for the interest on which annual provision must be made, is better spent upon railway work, which will at once increase the output of the primary products of Australia and provide work for an increased population. I would therefore urge that

an early decision should be arrived at with regard to the route of the transcontinental line, and that the work of construction should be immediately started. Mr. Hobler's scheme seems to satisfy all requirements and to involve the least capital expenditure and the least probable annual charge upon the Exchequer.

With regard to the unification of gauges, I think this work should be postponed, except in regard to two improvements which might be carried out at moderate expense. These improvements are the construction of a 4 ft. $8\frac{1}{2}$ in. gauge direct line from Port Augusta to Salisbury. The southern half of this line is already constructed, or is being constructed, on the 5 ft. 3 in. gauge, and this portion could easily be converted to a 4 ft. $8\frac{1}{2}$ in. gauge. The line from Salisbury through Adelaide and Melbourne to Albury should be maintained as it is on a 5 ft. 3 in. gauge. The second improvement would be the completion of the New South Wales coastal line from West Maitland to Richmond Gap; not very much work is required upon this, and the line is on the 4 ft. $8\frac{1}{2}$ in. gauge. A comparatively short new south line on this gauge from Brisbane to Richmond Gap would give an uninterrupted 4 ft. $8\frac{1}{2}$ in. gauge line from Albury to Brisbane. If these two improvements were made there would be a quite appreciable shortening of the total railway mileage between Brisbane and Fremantle, and there would be only three stations on the route of 3,356 miles of track where passengers would have to change trains—viz., Albury, Salisbury, and Coolgardie.

In this question of the unification of gauges British engineers might help their brethren in Australia by devoting serious attention to the problem of devising adequate mechanical means of coping with the difficulties brought about by break of gauge. If the loading and unloading of trucks at each break-of-gauge station could be obviated, the question of break of gauge would be a very unimportant one. As regards passenger traffic, it is not an important problem; it is only when heavy goods traffic has to be dealt with that the problem becomes a serious one from the point of view of working expenses and rates for transport of goods.

THE STUDY OF MAN.

ADDRESS TO SECTION H (ANTHROPOLOGY) BY

H. J. E. PEAKE,

PRESIDENT OF THE SECTION.

In all sciences there comes a time when it is well to pause and to take stock of our labours, to consider our position and to focus our attention upon our ultimate goal. Such a time seems to have arrived in the study of Anthropology, and, though it would have been better that the agent had been one with more right to speak for the science, this occasion seems not unfitting for the purpose.

During the last ten or twelve years a change has been creeping over the science, and the outlook has altered. Twelve years ago anthropologists in this country, with scarcely an exception, were devoting their energies to tracing out the evolution of customs, institutions, and material culture, assuming in all cases that, where similarities were found in different parts of the world, they were due to independent origins and development. It was assumed that the workings of the human mind were everywhere similar, and that, given similar conditions, similar customs and culture would originate and develop on the same lines. The evolution of civilisation was looked upon as a single line of advance, conditioned by the unalterable nature of the human mind, and it was supposed that barbarian and savage cultures were but forms of arrested development, and indicated very closely past stages in the history of civilised communities.

But during the last twelve years a fresh school of thought has come into prominence. According to this new view discoveries are made but once, and when resemblances are found between the cultures of different communities, even though widely separated, this is due to some connection between them, however indirect. According to the new school of thought the development of civilisation has been proceeding by many different paths, in response to as many types of environment, but these various advances have frequently met, and from the clash of two cultures has arisen another, often different, more complex and usually more highly developed than either of its parents.

The old school looked upon the advance of culture as a single highway, along which different groups had been wandering at varying paces, so that, while some had traversed long distances, others had progressed but a short way. The new school, on the other hand, conceives of each group as traversing its own particular way, but that the paths frequently meet, cross or coalesce, and that where the greatest number of paths have joined, there the pace has been quickest.

The older school, basing its views of the development of civilisation upon the doctrine of Evolution, has called itself the Evolutionary School.

The newer, while believing no less in Evolution, feels it a duty to trace out minutely the various stages through which each type of civilisation has passed by independent inquiry, rather than to assume that these stages have followed the succession observable elsewhere; thus, as historical factors form a large part of its inquiry, it has been termed the Historical School.[1]

The first note announcing the coming change was sounded from this chair eleven years ago,[2] and during the interval which has elapsed since then the new school has gained many adherents. All of these will not subscribe to the dictum that no discovery has been made twice, for was not the safety-pin patented early in the nineteenth century by someone who must have been ignorant that the same device had been employed at the lake-dwelling of Peschiera about 1400 B.C.? Nevertheless, there is a tendency at present not to assume an independent origin for any custom or device until it has been proved that such could not have been introduced from some other area where such custom was practised or such device known.

These tendencies have led the anthropologist to inquire more fully into the history of the peoples whose civilisation he is studying, and to note, too, minute points in their environment, which may have suggested customs or modifications in practice in use elsewhere. At the same time geography, which had been growing into a more living study, and ceasing to be a mere record of places and statistics, began in some centres to take special note of man and his doings. This anthropogeography concerned itself mainly with inquiring into the reactions between man and his environment, and though at first the environment was the main object of the geographer's attention, its effect upon man has become more and more pronounced of late. Thus anthropology and geography have been drawing closer together during the last few years, and as the latter is a recognised and popular subject in the curriculum of our schools, no small amount of anthropological knowledge has been instilled into the minds of our boys and girls, and with that knowledge a still greater measure of interest in the subject.

It might have been expected that before the geographers the historians would have been attracted to the anthropological approach, but recent political events have up to now engrossed the attention of most historical students. Signs have not been lacking, however, especially during the last year, that the study of peoples and their customs, rather than of kings and politicians, is gaining ground, and we may, I think, look with confidence towards closer relations between the studies of history and anthropology in the near future.

Again, we may notice an increasing interest in our subject among sociologists and economists. These have rightly focused their attention upon the social organisation and economic well-being of highly civilised communities such as our own, with a view to presenting an orderly array of facts and principles before our political leaders. There

[1] Rivers, W. H. R., 'History and Ethnology.' *History*, v. 65-7, London (1920).

[2] Rivers, W. H. R., 'The Ethnological Analysis of Culture.' *Report of Brit. Assoc.*, 1911, 490-2.

has, however, been a tendency during the past few years to trace these modern conditions back into the past, sometimes into the remote past, and to use for purposes of comparison instances drawn from the social organisation or economic conditions of communities living under simpler conditions. While these studies must, to some extent, overlap those of the anthropologist, their methods, and especially their points of view, are different. We are working from the simple to the complex; they begin with more highly developed conditions and thence work back to the more primitive.

Lastly, we must not forget the students of the classical languages. These studies have been severely attacked of late, and their value to humanity disparaged. In spite of many advantages which they possess at schools and universities, they have been losing in popularity, and the reason is not far to seek. So long as there were fresh works to be studied and commentated, and imperfect texts to be emended and elucidated, there was no lack of devotees to classical literature. This was the case in the eighteenth and early nineteenth centuries. Later on comparative philology gave fresh life to such studies, and there was work to be done in comparing Greek with Latin and both with other Aryan tongues; certain views current among mid-nineteenth-century philologists gave also an impetus to the re-study of Greek mythology. But about 1890 such studies became unfashionable in this country, and many classical scholars, at a loss for a line of research, turned to anthropology with great advantage both to themselves and to us. This movement was crystallised by the appearance of 'Anthropology and the Classics' in 1908, and since that date the two studies have kept in the closest touch.

It is doubtless as a result of these converging movements that the general public is taking an intense interest in anthropological studies at the present time, and that works of a general nature, summing up the state of knowledge in its different branches, are in great request by the general reader. The educated public, and many, too, whose opportunities for study have been more restricted, wish to know more of the Science of Man, yet I fear they are too often perplexed by the discordant utterances of anthropologists, many of whom seem to be far from certain as to the message they have to deliver.

In their turn not a few anthropologists feel a like uncertainty as to the ultimate purpose of their studies, and are far from clear as to how the results of their investigations can be of any benefit to humanity. These are points well worthy of our serious consideration; for, as we were reminded from this chair two years ago,[3] anthropology, if it is to do its duty, must be useful to the State, or at least to humanity in general. Even the scope of the science is by no means clear to all, and would be differently defined by various students. It may not be out of place, therefore, first of all to consider in detail the scope and content of anthropology, then its aims and the services it may render to mankind.

To the outside world anthropology seems to consist of the study of

[3] Karl Pearson; Address to the Anthropological Section. *Brit. Assoc. Report,* 1920, 140-1.

flint implements, skeletons, and the ways of savage men, and to many students of the subject its boundaries are scarcely more extensive. Yet civilised people also are men, and if anthropology is the science of man it should include these within its survey. That other scientists, such as historians, geographers, sociologists, and economists, study the doings of civilised man is no reason why the anthropologist should fail to take them into account, for his point of view is in many respects different from theirs. I would suggest, therefore, that all types of men, from the most civilised to the most primitive, in all times and in all places, come within the scope of anthropology.

That anthropology is the study of man is a commonplace, but we need a more accurate definition. A former occupant of this chair has declared that 'Anthropology is the whole history of man as fired by the idea of Evolution. Man in evolution—that is the subject in its full reach.' He adds: 'Anthropology studies man as he occurs at all known times. It studies him as he occurs in all known parts of the world. It studies him body and soul together.'[4]

Anthropology may, therefore, be defined as the study of the origin and evolution of man and his works, provided that we realise that the works of men's brains are as important to the anthropologist, even more important than the works of men's hands. What, then, separates anthropology from the other studies which are concerned with man and his many activities is, that the anthropologist studies man from all points of view—that his is a synthetic study; above all, that Evolution is his watchword; that his study is, in fact, not static but dynamic.

If, then, we grant that anthropology is the synthetic study of the evolution of man and his manifold activities, we are dealing with a subject so vast that some subdivision becomes necessary if we are to realise what the study involves. Such divisions or classification must, to some extent at least, be arbitrary, but in the first instance we may safely consider the subject as primarily divided into two main categories: 'man' and 'his works.'

But man himself, the human organism, cannot be considered from one aspect only, and various partitions have been made by theologians and philosophers. For his purpose it seems more fitting that the anthropologist should consider the division as twofold, that man consists of body and mind; the study of these is the special province of the anatomist and physical anthropologist on the one hand and of the psychologist on the other.

Here, again, it may be asserted that anatomy and psychology are distinct sciences, and in no way to be considered as parts of anthropology, and in a certain sense this is true. But anatomy, in so far as it helps us to understand the evolution of man from his simian ancestor, and again as it helps us to trace the variations in the human frame, and so to follow the movements of different types of men as they mingle with one another in successive ages, is and always has been reckoned a definite branch of anthropology.

Again, in the case of psychology, which has made such immense strides during the last few years, there is much which is not, strictly

[4] Marett, R. R., *Anthropology*, p. 1.

speaking, anthropological. On the other hand, in so far as psychology enables us to trace the development of the human mind from that of the animal, and in so far, too, as it can interpret the causes which have led to the various forms of human activity which meet us in different times and different places, so far is it a branch of our science. If, too, it can help us to ascertain whether certain fundamental mental traits, due perhaps to a long-continued environment in the far past, are normally associated with certain physical types, psychology will provide anthropologists with a means of interpreting many of the phenomena which they have noted but cannot fully explain. Much, therefore, which is included in the studies of anatomy and psychology may justly be included within the scope of anthropological studies.

The works of man are so numerous and varied that it is by no means an easy task to classify them. We may, however, first distinguish the work of man's hands, his material culture, from his other activities. Under this heading we should include his tools, weapons, pottery, and textiles; his dwellings, tombs, and temples; his architecture and his art. Nor need we confine our attention to their primitive stages alone, for as anthropologists we are concerned with their evolution from the simplest to the most complex.

Next, we have the problems concerned with language, which we may consider as dealing with the means whereby men hold intercourse with one another and communicate their wishes and ideas. This heading might well include gesture at the one end and writing at the other, for gesture, language, and writing all subserve the same end. Hitherto anthropologists have confined their attention too exclusively to the tongues of backward tribes, and left the speech of more advanced peoples to the philologists. I would plead, however, that language is such an essential element in human culture that comparative philologists might well consider themselves as anthropologists, and their studies as an important part of our science.

Lastly, we have social organisation and all that may be included under the terms 'customs' and 'institutions'; a varied group, leading on the one hand to the study of law, and on the other to that of comparative religion. Here, again, we come in contact with other studies—those of the lawyer, political economist, and theologian; but though the anthropologist is to some extent studying the same series of facts, his range is wider and his outlook more dynamic.

Thus it will be seen that in the three divisions of men's work, as well as in the two aspects of man himself, the anthropologist finds other workers in the field. But whereas these other sciences are concerned only with some part of man and his works, and limited frequently to recent times and civilised communities, it is the province of the anthropologist to review them as a whole, in all times and in all places, and to trace their evolution from the simplest to the most complex.

If we accept the views of the Historical School, anthropology becomes a new method of treating historical material. It is, in fact, the history of man and his civilisation, drawn not so much from written documents as from actual remains, whether of material objects or of customs

and beliefs. It is concerned with wars only so far as these have produced a change in the population or language of a region. It is interested in kings only when these functionaries have retained customs indicative either of priesthood or divinity. It is interested less in legal enactments than in customary institutions, less in official theology than in the beliefs of the mass of the people; the acts of politicians and diplomats concern it not so much as do the everyday habits of the humbler folk.

From some points of view anthropology may be considered as a department of zoology, but whereas other branches, such as entomology or ornithology, deal with classes containing innumerable species, anthropology deals with one family only, and that containing but one recent species; and, although this species has a number of varieties, these are fertile *inter se*. Many aspects of his subject, which occupy much of the attention of the zoologist, such as taxonomy and phylogeny, form but a small part of the anthropologist's inquiry; on the other hand, though the zoologist, when dealing with the higher groups, studies instinct, the nests and songs of birds, and the organisation of bees and ants, such inquiries are slight as compared with the corresponding problems which face the anthropologist.

A century ago zoologists were largely engaged in studying the higher groups of animals—vertebrates, insects, and the like—and for a time neglected the 'radiate mob.' Then there was a sudden change; all interest was focused upon lowly forms, and the protozoa occupied a disproportionate part of their attention. Lately, again, their work has been more evenly distributed over the whole field, and attention has been paid especially to those groups which most affect mankind for good or ill.

This choice of groups for special study was by no means due to mere caprice; there was a sound reason behind it. The more obvious forms of life were first studied; then, as microscopes improved, attention was focused for a time upon the simpler organisms; for, from the study of these, the zoologist was better able to grasp the underlying principles of life. These lessons learnt, he was able with greater certainty to attack the problems affecting the welfare of mankind.

So with the student of man. For many centuries historians, philosophers, and theologians have been studying the ways of civilised humanity, though not always quite by the methods of the modern anthropologist. For, just as they were attracted by the higher groups of men, so were they also fascinated by the more conspicuous individuals in those groups rather than by the general mass. During the nineteenth century students were attracted towards the backward types of humanity, partly, perhaps, because of their very unlikeness to ourselves, and of recent years largely because they felt that the customs of these primitive peoples formed most important scientific evidence which was fast disappearing. But from a scientific point of view the paramount reason was because it was felt that in such simple societies we should find the germ from which human civilisation had begun, and that we should there discover the ancestral form from which our modern culture had developed.

Much of the force of this last argument is disappearing as the

Evolutionary School gives place to the Historical. By degrees we are becoming aware that the civilisation of backward peoples is more complex than was at first believed; that they, too, have had as long a history as ourselves, even though it may have been less eventful. We are giving up the belief that such people are human fossils, and that they have preserved our ancestral types alive to the present day, for we are realising that they represent not so much our ancestors as our poor relations.

On the other hand, though, perhaps, we must abandon the ancestral view, and cease to believe that these backward communities represent accurately to-day the conditions under which we dwelt in long past millennia, the customs and institutions of these folk are in many respects less complex than our own, and it is possible to study them from every aspect with far greater ease than we could do in the case of one of the higher civilisations. Since it is one of the functions of anthropology to study man synthetically, this is a great advantage. When dealing with these simpler problems we can evolve a method and a discipline to be applied in more complicated cases. Again, the backward peoples have, as a rule, no written history, and we are forced in this case to restore their past by other means. This has led to the development of fresh methods of attacking the problems of the past, which may prove of value in the case of more advanced communities, where written evidence exists, it is true, but is, to some extent at least, faulty, imperfect, or unreliable.

For these reasons the study of backward peoples still has great value for the anthropologist. He has not yet solved all the problems concerned with the dawn of civilisation, nor has he yet perfected his methods and discipline. Although vast quantities of observations, good, bad, and indifferent, by trained workers and dilettante travellers, have been placed on record, especially during the last half-century, there is much more to be collected from this fast disappearing mass. More workers and expert workers are needed in this field, and so it is that our universities devote the greater part of their energies to training students for this purpose.

But there are many students, equally interested in the evolution of man and his works, who cannot, for one reason or another, visit wild lands to study the ways of their inhabitants. Some of these, it is true, may sift and arrange the vast mass of material collected by their more fortunate colleagues, though they will be at considerable disadvantage when undertaking this work if they have had no personal experience of the lands and the people with which their material is dealing.

The time seems to have arrived when anthropologists should not concentrate so exclusively upon these lowly cultures, but might carry on their researches into those civilisations which have advanced further in their evolution. Not that I wish to be understood as deprecating in any way the study of backward peoples, or as discouraging students from researches in that direction. But I would suggest that the time has arrived when some anthropologists might initiate a closer inquiry into the conditions of more civilised peoples, not in the place of but in addition to the studies already described.

Quite apart from such states of society as are neither wholly primitive nor entirely advanced, we have in the Old World three great centres of culture, each of which has in its turn been in the van of progress, and each of which has contributed no little to the advance of the others. These are the civilisations of China and the Far East, of the Peninsula of Hindustan, and what, for lack of a better term, I must call the European Region.

Though our relations with China and Japan have been intimate, and on the whole friendly, for several generations, and many of our compatriots are fairly familiar with both countries and their languages, it is surprising how little we know of either of these people from the strictly anthropological standpoint. This is the more to be regretted since for over half a century Japan has been undergoing a change and adopting fresh features from Western civilisation, while there are signs that the same movement is beginning to take place in China. So far those who have had an opportunity of working in these countries, and have made themselves familiar with the languages of the Far East, have studied the art, literature, philosophy, and religion of these regions, rather than those aspects which more properly belong to our subject. I have no desire to minimise the value of such studies, but as part of the science of man we need to know more of the physical form and mental traits of these people, more, too, of their ordinary material culture as it was before it came into contact with and borrowed from the West, more of the dialects spoken in their provinces, and particularly more about their social and territorial organisation, and about the beliefs and superstitions which have survived alongside of their official religion. Certain fragments of such information are, it is true, to be found among the writings of Westerners who have lived long in the East, but there are so many gaps in our knowledge that it is not easy to piece them together into an intelligible whole.

What concerns us more nearly in this country is the Indian Region. Here we have a well-defined province, peopled by successive waves of different races, speaking different languages, and with different customs and beliefs—an apparently inextricable tangle of diverse elements in various stages of cultural evolution. A vast amount of material has been gathered in the past, though such collecting has not been proceeding so fast during the last generation; but basic problems are still unsolved, and seem at times well-nigh insoluble. Perhaps it is this superabundance of material, or it may be the apparent hopelessness of the task, which has diminished the interest taken in these studies during the past few years. This attitude is regrettable, and the only redeeming feature is the extremely active and intelligent interest in these problems now taken by various groups of Indian students, especially in the University of Calcutta.

I have suggested that perhaps the lack of interest in such matters among Anglo-Indians, and especially among members of the Indian Civil Service, may be due to the apparent hopelessness of reaching a solution of any of the problems involved. It may also be due to the fact that they are sent out from this country to govern a population with different cultures and beliefs, and traditions wholly unlike those

of this continent, without having received in most cases any preparation which will enable them to study, appreciate, or understand an alien civilisation. Thus, with the best of intentions, they misunderstand those among whom they are sent, and are in turn misunderstood. Guiltless of any evil intent, they offend the susceptibilities of those among whom their lot is cast, and acts are put down to indifference or ill-nature which are only the product of ignorance. After making their initial mistakes the more intelligent and well-meaning set to work to study the people committed to their charge, but faced with problems of extreme intricacy, and without any previous training, more often than not they give up the attempt as hopeless.

That candidates for the Indian Civil Service should receive a full training in anthropology before leaving this country has been pleaded time after time by this Section and by the Anthropological Institute, and though I repeat the plea, which will probably be as useless as its predecessors, I would add more. The problems confronting the anthropologist and the administrator in India are of such extreme complexity that it needs a very considerable amount of combined action and research even to lay down the method and the lines along which future inquiries should be made. Such a school of thought, such a nucleus around which further research may be grouped, does not yet exist; the materials out of which it can be formed can scarcely yet be found. And yet until such a nucleus has been created, and has gathered around it a devoted band of researchers, no true understanding will be found of the problems which daily confront both peoples, and the East and the West will remain apart, subject to mutual recriminations, the natural outcome of mutual misunderstanding.

One solution only do I see to this dilemma. For many years past there have been institutions at Athens and Rome where carefully chosen students, with the best of qualifications, have spent several years studying the ancient and modern conditions of those cities and their people. By this means a small but well-selected group of Englishmen have returned to this country well-informed, not only as to the ancient but the modern conditions of Greece and Italy. Besides this we have had in each of the capitals of those two States an institution, subserving no political or diplomatic ends, which has acted as a centre or focus of research into the civilisation of those countries. Although the main objects in both cases have been the true understanding of the cultures of the distant past, the constant intercourse of students of both nationalities working for a common end has resulted in a better understanding on the part of each of the aims and ideals of the other. I have no hesitation in saying that the existence of the British Schools at Athens and Rome has been of enormous value in bringing about and preserving friendly relations between the people of this country and those of Greece and Italy.

I cannot help feeling that a similar institution in India, served by a sympathetic and well-trained staff, to which carefully selected university men might go for a few years of post-graduate study, would go far towards removing many of the misunderstandings which are causing friction between the British and Indian peoples. Such a British School

in India, if it is to be a success, should not be a Government institution, but should be founded and endowed by private benefactors of both nationalities. It would be a centre around which would gather all anthropological work in the peninsula, while it would enable the British students to arrive at a truer understanding of Indian ideals and help Indians to grasp more fully the relations subsisting between Indian and European civilisations.

Lastly, we come to that great area which I have termed the European Region, extending southward from our continent to the southern confines of the Sahara, and eastwards to Mesopotamia and beyond. Throughout all this vast region the racial basis of the population is similar, though the proportion of the elements varies. Also throughout the region there has been, from the earliest days of which we have evidence, free communication and no great barriers to trade and migration.

Until the last fifteen hundred years the civilisation of this area was fairly uniform, though its highest and earliest developments were in the south-east, while the northern zones lagged behind and were on the outer fringe. Still, with the possible exception of North Russia, it formed from palæolithic times one cultural region, and this became more marked and homogeneous during the flourishing days of the Roman Empire. Two forces from without, coming from the outer fringe of this region, destroyed that mighty empire and divided the region into two halves; and as each of these forces adopted different religious views, the European cultural region became divided into two. For many centuries these sections were at war with one another, and their boundary oscillated; the East pushed westward until 1500 A.D., and since then has been in retreat. We have, therefore, during recent centuries to treat the European cultural region as two, the civilisations of Islam and Christendom.

Though the separation of these two halves is relatively recent, their ideals have grown more and more divergent, while the inhabitants of both zones, though no longer in constant warfare, are no nearer to a true understanding of one another. Political difficulties in the Near East, which show no signs of diminishing but seem rather to be on the increase, are the natural result of such misunderstandings, and the remedy here, as elsewhere, is to achieve a truer appreciation of other points of view, due to a divergence in the evolution of civilisation, due in its turn to a different environment. A more thorough knowledge of the anthropological factors of the case seems to be a necessary preliminary to such mutual understanding, and since the League of Nations and the Versailles Treaty have seen fit to add to our responsibilities in this area, it is an urgent necessity that some of our anthropologists should pay a closer attention to the problems of the Near East.

And now with regard to Christendom. Are we to consider that our duties as anthropologists end with alien cultures? Is Christendom so united that misunderstandings cannot arise within its borders? At the close of a great war, which divided this area into two hostile camps, we can hardly claim that there is no room for our studies.

As we have seen, there has been a tendency hitherto to regard

anthropology as a science dealing with primitive and backward peoples, and it has sometimes been felt that to apply its principles to neighbouring peoples, enjoying as high a civilisation as our own, might be looked upon as an insult, implying that their culture was sufficiently primitive to warrant their inclusion in our inquiries. But if we agree that all mankind, savage and civilised alike, are fit material for the anthropologist's investigations, we need have no hesitation in studying, not only the bodily and mental equipment of our neighbours, but their material culture, social organisation, and religious beliefs, just as already, for practical purposes, we study their languages.

To some extent this has been done by travellers, who describe strange customs and ceremonies which take place in out-of-the-way places. These are usually, however, selected because they are quaint or rare, rather than for the scientific value which they possess, and being recorded too often by untrained observers many details of the utmost scientific importance are frequently omitted. In spite of the comparative uniformity in customs and beliefs among the educated classes throughout Christendom, a uniformity which is perhaps more apparent than real, as soon as we get to the peasant or workman the differences become more apparent. There is not a country in Europe, nor even a province, in which we may not find features of an anthropological nature which separates its population, in some respects at least, from the inhabitants of other areas. It is these differences, unimportant as they may appear, which come to the front when trouble is brewing, and these are the factors which, above all others, we need to understand if we are to avoid treading on corns in moments of national irritation.

It does not fall to the lot of many to spend part of their lives among backward peoples, and only a small section of our compatriots dwell amid the civilisations of the East. Many people, however, have constant opportunities of travel throughout Christendom, and not a few visit from time to time some of the lands in the Islamic zone. Here they can, to some extent at least, obtain first-hand data of an anthropological nature, and make themselves familiar with some aspects of the life of the place. With minds trained to observe accurately and to understand what they see, even a few weeks' holiday in a foreign land will enable them to appreciate better the ideals of their hosts. Constant travel by people alive to the importance of such inquiries will in time so influence the public opinion of many of the nations of Europe that misunderstandings will be less frequent and national sensitiveness less prone to take offence at words and actions which are not intended to provoke.

But it is not only foreign countries and their inhabitants which the anthropologist needs to study. In every country, especially in lands which have been subject to successive invasions, there are different strata in the population which have different customs and a different outlook. The British Isles are no exception to this rule; history records the successive arrivals of Romans, Saxons, Danes, and Normans, and the study of prehistoric remains shows us that these invasions have been preceded by a greater number in earlier days. Just as the physical

type of the Briton is far from uniform, so is his mental outlook and his ideals and beliefs. Quite apart from the differences observable in the different countries which compose our group of islands, and the different provinces into which they have been or may be divided, we find also, in any given area, that the population insensibly divides itself into groups or classes, differing but slightly except in name and the absence of rigidity from what we know in India as castes. These classes in the British Isles have had their origin not so much in economic conditions as in the successive waves of conquest which these islands have suffered. Individuals, it is true, have freely passed from one class to another during the nine centuries which have elapsed since the last conquest, but though the individuals have changed the classes have remained. Owing to the constant interchange in blood the physical characters of the different classes are much alike, as are their fundamental mental traits, but in material culture, language, social organisation, and to some extent religious beliefs, they differ widely.

Here then again, in our own country, there is work for the anthropologist. Here are various groups, how many it is at present difficult to say, not clearly distinguished from one another by a sharp dividing line, and intermixed in the same areas, yet groups which are to the anthropologist separate units which require distinct study. Even among the richer and better educated sections of the community, who have mingled together in social intercourse for several generations and whose families are allied by marriage, we may find differences of outlook, according to the type of tradition handed down in the family. The outlook and ideals of landed or territorial families differ from those of the mercantile class, nay even the merchants and manufacturers have in many ways distinct traditions which are handed down from one generation to another. So that even in such a group as we find assembled at the meeting of the British Association, who have come together with one end in view, the advancement of science, we shall find, were we to analyse the feelings and opinions of the different individuals, that owing to differing traditions, handed down through many generations, their views on social and religious questions are fundamentally unlike; that they belong, in fact, to many distinct anthropological groups. There is work, then, for the anthropologist who never leaves these shores.

Turning now to the aims of anthropology and to the means whereby it may become utile to the State and to mankind in general, we see that it is of the utmost importance that those who are sent to govern or administer areas and districts mainly occupied by backward peoples should have received sufficient training in the science to enable them, in the shortest possible space of time and consequently with the fewest possible initial mistakes, to govern a people whose customs, traditions, and beliefs are very different from their own, without offending the susceptibilities of their subjects.

We are an Imperial people, and during the last few centuries we have taken upon ourselves a lion's share of the white man's self-imposed burden, and the lives and well-being of millions of our backward brethren have been entrusted to our charge. Recent events have,

by means of Mandates, added largely to our responsibilities in this respect. We, of all nations, cannot disregard this fundamental duty of dispatching our pro-consuls fitted to undertake on our behalf these great responsibilities.

But the burden we have undertaken extends not only to backward peoples; we have been called upon, in one form or another, to govern or to advise the Governments of peoples who have, or have had in the past, a civilisation little, if at all, inferior to our own, and to whom at one time we have been indebted, directly or indirectly, for much of the culture that we now enjoy. The civilisations of these regions are infinitely more complex, and, as is always the case in civilised areas, the people are not homogeneous, but are divided into numerous sections, differing in language, religion, and social customs. In these regions we meet with anthropological problems of infinite difficulty and complexity, on the solution of which depends the peace and well-being of the population. And yet our representatives go to take up their duties in these lands with little or no previous training, and it is only a marvel that errors of tact, due to ignorance, are not more common.

In these civilised regions race consciousness has been growing fast during the last half-century, and errors of tact and manners, which were submitted to in former times, though not with a good grace, are now actively resented, and the old methods of government are discredited. It may not yet be too late to remedy this evil, if no time is lost in giving a full anthropological training to those who are sent to administer these regions.

But we are not only an Imperial people, governing and administering these regions with alien populations; we are also a wandering and adventurous people. The nomadic spirit of our ancestors is still alive within us; our ships, like those of the Vikings of old, are to be seen in every sea. So it comes that our people, whether travelling for pleasure or for purposes of trade, or serving in the Army or Navy, will be found in all lands and all climates from the Arctic circle to the Equator.

All these wandering Britons come in contact with the inhabitants of the lands they visit, creating various impressions, sometimes good, more often bad. Had they a fuller knowledge of the customs and opinions of the people they visit, or even a truer appreciation of the fact that diverse customs and opinions exist and should be respected, we should not have to record the creation of so many bad impressions. Luckily our people, as a rule, have much common sense, and often a desire to please, so this trouble is thus to some extent mitigated; but the difficulties that have arisen and are constantly arising from ignorance of the ways of others, from too insular an outlook, in fact, from a lack of appreciation of the anthropological standpoint, are making us and our government heartily disliked in nearly every quarter of the globe. It is to remedy these difficulties, and the danger to the peace of the world which is threatened thereby, that I would advocate an increased study of anthropology by all sections of the community. Herein lies one of the chief means by which our science may become utile to mankind.

It is not my business to draft a scheme for the furtherance of anthropological studies. Two of our universities offer degrees in this subject, and others a diploma; courses of instruction on some sections of the subject are given there and elsewhere. Many teachers of geography are introducing much anthropological matter into their curricula, and there are signs that some historical teachers may follow suit, so that the subject-matter, if not the name, is not unknown in some of our schools. But we have much lost time to make up and the matter is urgent.

We cannot, of course, expect all our people to be trained anthropologists and to understand fully all the ways of the people they may chance to meet in their wanderings. What matters far more is that they should appreciate the fact that different peoples have had different pasts and so act differently in response to the same stimuli. Further, that all this diversity has its value; that we cannot be sure that one culture is in all respects superior to another, still less that ours is the best and the only one which is of consequence. It is not so much the facts that matter as the spirit of anthropology; we need not so much that our people should have anthropological knowledge as that they should learn to think anthropologically.

It is needless for me to remind you that the world is in a state of very unstable equilibrium—that the crust is, so to speak, cracked in many places, and that the fissures are becoming wider and deeper, and that fresh fissures are constantly appearing, not only in distant lands but nearer home. Again, this crust, if I may continue the geological metaphor, is stratified, and there are horizontal as well as vertical cleavages, which are daily becoming more marked. It is to the interest of humanity that these breaches should be healed and the cracks stopped, or we may find the civilisation of the world, which has grown up through long millennia at the cost of enormous struggles, break up into a thousand fragments. Such a break in the culture of the European Region followed the dissolution of the Roman Empire, and more than a thousand years were needed to heal it; nay, some of the cracks then made have never yet been closed.

Anything that may help to avert such a disaster is important to the human race, and there is no greater danger at present than the alienation of the peoples of Asia and the Near East. Much of the ill-feeling engendered in India, Egypt, and elsewhere is the product of misunderstandings, due to a lack of appreciation on both sides of the opinions and views of the other party, and there seems to be no better method of removing such misunderstandings than a sympathetic study of one another's culture, and to this end anthropology offers the most hopeful approach.

THE EFFICIENCY OF MAN AND THE FACTORS WHICH INFLUENCE IT.

ADDRESS TO SECTION I (PHYSIOLOGY) BY

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THE subject of my address—the efficiency of the human organism and the factors which influence this efficiency—is, in my opinion, one of the most important problems of the present day. It is a problem which cannot, however, be considered only from its physiological aspect if it is to receive adequate consideration; its implications are much wider, reaching right down to the very basis of our daily lives. As I am no expert in industry or economics, I shall confine my attention as far as possible to the problem from the physiological side, and leave to others the sociological application.

The term ' efficiency ' has become a mere catchword, bandied about by people who have not the faintest idea of what the word connotes. Practically it has come to mean, to the average man in the street, the mythical improvement which is to be anticipated from some change in workshop or office organisation—a bigger and better result at a smaller cost. The word has a very definite meaning in engineering science, and this meaning has been transferred from the inanimate machine to the living organism. In the case of the engine the problem is relatively simple, as the number of interfering factors is not great, but the solution of the problem in the case of the organism is beset with many difficulties, as the interfering factors are numerous and varied. Two types of efficiency are spoken of in connection with the animal body. One type is the mechanical efficiency in the engineering sense, *i.e.* the ratio which exists between the heat equivalent of the external muscle work done and the energy output of the subject during the performance of the work in question. This is a problem which has attracted many workers, and there seems to be a general consensus of opinion that the efficiency of man in the performance of external work is about 20 per cent. gross and 25 per cent. net. (*Gross* efficiency is obtained by dividing the actual heat equivalent of the external work done by the total output of energy of the man during the time occupied in the performance of the external work, and *net* efficiency is obtained by dividing the heat equivalent of the external muscle work done by the actual increase in the energy output of the subject over the basal energy output during the performance of the work in question.) As A. V. Hill has pointed out, this striking unanimity is in

all probability due to the fact that the maximum value of efficiency remains more or less constant over a fairly wide variation in the mode of the performance of the work. The work referred to here is wholly concerned with muscle activity. The other type of efficiency is that which is called industrial or productive efficiency, where the capacity of the individual to perform effective work is dealt with, judging the capacity of the individual by, for example, his output in unit time. So far as the worker himself is concerned, the whole object in industrial efficiency is undoubtedly to get the greatest output with the minimum of effort. The determination of the mechanical efficiency is fairly readily carried out, but it is very difficult to get an accurate gauge of the industrial efficiency. At bottom they are closely related, and both are physiological problems.

The leaders of industry have not been slow to accept and utilise the gains of science in the realm of inanimate things, but they have been slow to recognise the fact that there is a science of physiology which deals with the man who controls the productive machinery. New inventions may completely revolutionise shop equipment, good machines may be replaced by better, and better by still better, but man remains almost as immutable as the ages. Physiological evolution is infinitely slow. As Lee puts it, 'Try as we will we cannot get away from the fact that so long as machines need men, physiological laws must be reckoned with as a factor in industrialism.' Butler in 'Erewhon' satirised in his inimitable way this very problem of the industrial world as follows: 'So that even now the machines will only serve on condition of being served, and that upon their own terms; the moment their terms are not complied with they jib, and either smash both themselves and all whom they can reach, or turn churlish and refuse to work at all. How many men at this hour are living in a state of bondage to the machines? How many spend their whole lives, from the cradle to the grave, in tending them night and day? Is it not plain that the machines are gaining ground upon us, when we reflect on the increasing number of those who are bound down to them as slaves and of those who devote their whole souls to the advancement of the mechanical kingdom? . . . May not man himself become a sort of parasite upon the machines? An affectionate machine-tickling aphid?'

It is a clever picture, and if one looks back on the rise of industrialism it is not so very far-fetched. It is but a little more than a hundred years since this country was industrialised, and we are still reaping the aftermath of the terrible conditions which then reigned, when the great centres of industry were swamped with country dwellers who poured into the towns in the race for wealth. Few realise the hopelessly unphysiological conditions which developed in the methods of work, the hours and conditions of work, the housing. Many talk now of the hardship of the eight hours' day under conditions which are relatively civilised, where the place of labour and the methods and machinery used are supervised by skilled and honest Home Office inspectors, where child labour is firmly controlled, and where practically all abuses are checked. The following citation from Robert Owen, that shrewd, gullible, high-minded, wrong-headed, illustrious and

preposterous father of Socialism and Co-operation,' as Lytton Strachey calls him, gives a good idea of the conditions ruling in the early years of last century in one of our staple industries. 'In the manufacturing districts it is common for parents to send their children of both sexes at seven or eight years of age, in winter as well as summer, at six o'clock in the morning, sometimes, of course, in the dark, and occasionally amidst frost and snow, to enter the manufactories which are often heated to a high temperature, and contain an atmosphere far from being the most favourable to human life, and in which all those employed in them very frequently continue until twelve o'clock at noon, when an hour is allowed for dinner, after which they return to remain, in a majority of cases, till eight o'clock at night.' Six till eight, with a break of one hour: a fourteen hours day, and fifteen was not unknown. Owen, in the article from which I have quoted, was petitioning Parliament, asking what? That a twelve hours day be instituted, to include one and a-half hours for meals, and that no child should be employed until the age of ten was reached. He pointed out in the course of the article that the results from the manufacturers' point of view would be better with a twelve hours day (*i.e* that the industrial efficiency, in modern words, would be improved).

Yet we wonder that the offspring of stock descended from workers under these conditions, which certainly improved as the century advanced but were far from ideal, gave the high yield of C3 lads recorded in the National Service Report. We might have been prepared for the disclosure, as the pre-war records of countries with Conscription showed that the number of rejections for the Army of town and factory workers was far in excess of those for men drawn from country districts. But evidence of the state of the national physique is not confined to these war figures. Sir George Newman, in his valuable and interesting Report on Preventive Medicine, has drawn attention to the enormous amount of time which is annually lost through sickness. The minimum average amounted to 14,295,724 weeks (or a period of upwards of 270,000 years) of sickness per annum, and this figure did not include absence from work due to maternity benefit, sanatorium treatment, or absence for less than four days per patient. This is the evidence of the National Health Insurance.

The design of the organism which has to stand the strain is not at fault. It is an organism which, in the language of the engineer, is abundantly supplied with factors of safety, and has an over-all high factor of safety. The body is not designed merely to perform the minimum amount of work or to stand the minimum strain; there is always a reserve. We have a circulatory system which is beautifully balanced to meet a strain, a system of vessels whose calibre can be increased or diminished so that the blood may be mobilised at the tissues or organs which require it, and a heart which has the capacity, provided it is normal and healthy, of responding to work, whose rate may be trebled in a few seconds when oxygen must be obtained and carbon dioxide got rid of. Not only can the amount of blood which is passed through the lungs during hard work be increased some five times, but the amount of oxygen taken in may

rise ten times. Thus the subject studied by Benedict and myself had a normal consumpt of about 200 c.c. oxygen per minute, and in one experiment he kept up an intake of nearly 2,000 c.c. per minute for four hours and twenty-two minutes on end. Quite often the same subject used 2,700 c.c. to 3,000 c.c. for ten minutes at a time. Again, at rest less than a third of the oxygen present in the blood is required, and even in the very hardest work the arterial blood is not depleted of its oxygen; it probably still contains more than a fourth. Curiously enough, the actual effectors, the muscles, do not of themselves seem to have a very high factor of safety. The structures, bones, and cartilage, to which they are attached, and which limit their action, and the amount of strain to which they can be exposed or subjected, have a very high factor of safety. A further protective mechanism for muscle is the perfect co-ordination between the groups of muscles, the elucidation of which problem we owe largely to our President, Sir Charles S. Sherrington. We have another reserve of first-class importance—viz., that when the strain on one group of muscles is becoming too severe, more and more groups of muscles are brought into action to help in meeting the strain, until in the end, if need be, practically the total musculature of the body is involved. And behind all this there are final factors of safety such as fatigue, which is a protective warning; and finally, if the latter be not heeded, collapse.

This perfect co-ordination of the different parts of the organism is required, because the human being is capable of intense muscular exertion for short periods. The intensity of the work is as a general rule inversely proportional to the length of time during which it must be carried out. The following table (Table I.) gives some idea of what is probably about the maximum effective muscular work per minute (modified from Blix):—

TABLE I.

Nature of Work	Duration of Work	Effective Muscular Work per min.	
		Kilogrammètres	Calories
Mountain climbing, moderate	Many hours	500	1·16
Mountain climbing, severe	1 to 2 hours	750–1,000	1·74–2·33
Mountain climbing, very severe	3·75 minutes	2,000	4·65
Treadmill	30 seconds	2,400–3,600	5·58–8·37
Running upstairs, 10 Kg. load	15 seconds	3,700	8·61
Running upstairs, no load	30 seconds	4,300	10·00
Running upstairs, no load	4 seconds	5,700–6,000	13·26–13·95

If, in the human organism, we were merely concerned with the co-ordinated action of a series of effectors, with the capacity of a certain group of muscles to perform a given amount of work, the solution of the problem would be relatively simple But we are dealing with a living organism capable not only of doing work but of repairing the worn-out parts as and when required. Further, we are dealing with an organism which varies not only in its capacity to perform work, but in its 'will to work.' We are dealing with a subtle organism which

has a whole series of protective mechanisms at its command, an organism which can be fatigued and rendered useless, as a working unit, by an amount of work on a particular day which on another day it can perform with the utmost ease and without apparent fatigue. We are dealing with an organism which can and does perform real hard muscular work with vigour and joy, and yet, if the nature of the employment or the environment be distasteful, can be reduced to impotence by work capable of being done by a child.

Again, the efficiency of a man is not merely dependent on the amount of work which can be performed by his muscles; the circulatory, respiratory, and nervous systems are of equal importance, and all are intimately related. The muscles must receive an abundant supply of blood, not merely to bring nutriment but to remove waste; there must be an efficient exchange of gases in the lungs, the rate of the respiratory and cardiac movements must be adapted to the work in hand through the co-ordinating agency of the central nervous system. Not only so, but, if the man is to work with the minimum of waste energy, there must be proper co-ordination between the various groups of muscles. A man does not walk, for instance, by the aid of his leg muscles alone, his lumbar muscles are equally important. Further, it is not a mere question of autonomic reflex adjustment, important though this may be, for much of the work done the attention must also be invoked. Yet, in spite of the many and varied stresses and strains to which the organism is subjected in the course of life, as the result of the many factors of safety, unless the overloading is excessive, too frequent or too long continued, the organism, so long as it remains physiological, is practically unaffected by ordinary hard work.

If we turn now to the consideration of the factors which influence the efficiency, both in the mechanical and the industrial sense, we find that the main controlling factor is undoubtedly the condition known as fatigue. Fatigue is a word just as frequently used as efficiency, and yet it is almost impossible to give an accurate definition of the term. Generally speaking, it is to be regarded as the antithesis of efficiency. As Vernon put it, 'By so much as fatigue is avoided or eliminated in industrial operations the efficiency of the worker is increased.' Fatigue may be summarised as a diminished capacity for doing work. The question of the site at which fatigue is first manifested, whether it is a central or a peripheral phenomenon, whether it is a specific condition, or whether, as Crile maintains, there is no ultimate difference between the bloodless intangible causes of fatigue and exhaustion and the bloody tangible causes of 'shock,' lies without the scope of this address. One of the great difficulties in the solution of the question is that no one has as yet devised a method which permits of a quantitative determination of the degree of fatigue. Indeed, some workers, Muscio for example, have definitely stated that such a test is an impossibility.

The study of the metabolism has given little or no clue so far. Benedict and I carried out a certain amount of experimental work on this phase of the question. Our results show that the subject may be on the very verge of absolute collapse, and yet, so far as the metabolic determination goes, there is no very marked evidence of diminished

efficiency in a mechanical sense. In an experiment with M.A.M., who, in the postabsorptive state, rode on a bicycle ergometer for nearly four and a-half hours until on the verge of collapse, doing 208,000 kilogrammètres of external work during the time, the metabolism was determined six times during the riding period with the following result:—

TABLE II.

Time	Oxygen Consumption per min. in c.c.	Rate of Work. Revs. per min.	Net Efficiency in per cent.
8.30 A.M. (start)			
9.00 ,, . . .	1,967	91·3	23·1
9.45 ,, . . .	1,946	91·4	23·3
10.30 ,, . . .	1,969	91·7	23·2
11.15 ,, . . .	1,948	90·3	23·2
12.00 noon . . .	2,003	89·0	21·7
12·45 P.M. . . .	1,899	78·2	21·3

It will be noted, as might be expected, that there is some slowing of the rate at which the work is done, but the diminution in the net efficiency, in spite of the fact that the subject admitted he was completely done at the conclusion of the last determination, is not striking.

In other experiments where the type of muscle activity used was marching, little apparent effect on the metabolic cost was noted until extraneous muscle activity was introduced in the form of staggering as the result of exhaustion.

Obviously, then, the capacity to carry on is limited by the genesis of fatigue. But it is equally obvious in practice that a man may be engaged in strenuous labour for many hours without acute signs of impending exhaustion. How is this condition attained? There are at least four factors which, to my mind, play predominant rôles in the attainment of maximum efficiency—viz., the rate of the performance of work, the amount of rest offered or taken by the subject, the rhythm with which the work is performed, and the work habits developed by the worker. Although I shall attempt to examine each of these factors separately, it is not to be inferred that they can really be considered as independent phenomena. As a matter of fact, they are all intimately related, and usually merge into one another.

Of these four factors probably most attention has been devoted to the rate or speed at which work is carried out. The glorification of that much misused half-truth, 'Time is money,' is responsible for much false physiology. Farmer, in a recent report to the Industrial Fatigue Board, laid, I think, the correct stress on the relation of speed to general industrial efficiency when he wrote: 'No movement can be compared with another and said to be better than it merely on account of its speed; it should only be compared in respect to ease and final result.' This is a good answer to those who believe that maximum efficiency can be best obtained by mere speeding up. Goldmark also stresses this aspect of the question. She writes: 'Now just in proportion as this function of speed is developed, subject to the capacities

of the human agent instead of as a driver of these capacities, it counts as a gain. Just so soon as the function of speed is dissociated from its effects on the worker we revert to the old system of pace-making and speeding.'

These are the observations of field workers. Can they be substantiated by experimental work in the laboratory? Benedict and I found, for example, working with a carefully calibrated bicycle ergometer, that there was a very close connection between the speed at which work was done and the mechanical efficiency. There was a very definite falling off with increased speed, as the following table shows. Unfortunately it was impossible to get our subject to pedal slower than 70 revolutions per minute.

Table III.

Revolutions per min.	Gross Efficiency	Revolutions per min.	Gross Efficiency
70	20·6	110	17·6
80	20·0	120	16·9
90	19·2	130	16·1
100	18·4	—	—

We found further that if the amount of effective muscular work done was kept constant, the efficiency fell with an increase of speed. Thus with effective work equivalent to 1.95 calories performed at the rate of 90 and 124 revolutions per minute respectively with the lower speed, the net efficiency was 22.6 per cent., whereas with the higher speed it fell to 15.7 per cent. Or again, with effective work of 1.58 calories at 71 and 108 revolutions per minute the efficiency was 24.5 per cent. and 15.6 per cent. respectively; and finally, with effective work of 1.35 calories at speeds of 71, 94, and 105, the efficiencies were 23.1, 20.4, and 17.0 per cent.

A. V. Hill has also recently dealt with this problem in a most interesting piece of work, where the activity was strictly confined to the biceps and the brachialis anticus. He demonstrated very clearly that, in spite of the fact that the slower the contraction the greater was the amount of work done, all the advantage thus gained was rapidly neutralised and dissipated as the result of the slow contraction necessarily causing an increased degradation of energy in the way of physiological changes resulting from the maintenance of contraction. It thus followed that a slow contraction, powerful though it might be, was not necessarily one of high efficiency. The actual efficiency, *i.e.* the ratio of the external work done to the energy degraded in carrying it out, was found to pass through a definite maximum value as the duration of the contraction increased. The maximum efficiency in his series of experiments was 26 per cent. He found that it was very rapidly attained, the optimum for the muscles investigated being apparently just under one second, but the fall which followed, as the duration of the contraction increased, was a comparatively slow one. On account, therefore, of the blunt nature of the curve the efficiency remained more or less constant over a wide range of speeds.

The load has obviously a direct connection with the speed at which work is done, but it has also a relation to efficiency. Benedict and I found, for instance, that both the gross and net efficiencies within the limits of our experiments increased with the load. The probable explanation of this result is that when light work is carried out, maintenance or physiological requirements which have to be covered form a large proportion of the total energy output, a balance which is steadily altered as the amount of external effective work done increases. Incidentally, Hill drew attention to a most important factor in the consideration of the efficiency of muscle, viz., the relation between the maximal and the submaximal effort. Hill suggested that the less powerful effort was the result of the maximal contraction of only a portion of the muscle fibres, and that the fibres not directly involved in the contraction changed passively, *i.e.* they were made to conform to the shape of their active neighbours. This, of course, will automatically lead to a considerable waste of energy in changing the form of the muscle as a whole, therefore the submaximal effort will be less efficient than a maximal effort of the same duration in time, and further 'the highest efficiency of a submaximal effort is obtained in a slower contraction than that of a maximal effort.'

On the other hand, when the loads become excessive there is a definite falling off, both in gross and net efficiencies. Laulanié, who also investigated this question, found that at voluntarily selected speeds, with steadily increasing load, the external work done rose with decreasing speed until the load became excessive. He maintained that there were two optima, (*a*) an economic optimum at 4 kilo. load with high efficiency and a low oxygen consumption per kilogrammètre, and (*b*) a mechanical optimum between 8 and 12 kilo. load when the output in unit time was highest. The following table from Laulanié makes his point clear:—

Table IV.

Resistance in kilos.	1	2	3	4	5	6	8	10	12	15
Speed adopted, mètres per sec.	1·49	1·07	0·80	0·61	0·54	0·44	0·37	0·29	0·24	0·13
Work done, kilogrammètres per 5 min.	448	642	726	778	812	853	**896**	**905**	**906**	570
Oxygen intake in c.c. per kgm.	3·5	2·44	2·17	2·14	2·23	2·25	2·43	2·53	3·12	5·31
Efficiency per cent.	14·1	20·4	22·9	**23·3**	22·3	22·1	20·4	19·7	17·0	9·4

It will be noted that when the load becomes excessive the efficiency rapidly falls away. This means that, although the effort may be continued as strenuously as before, and although the physiological cost of the effort remains at a very high level, the amount of external work done is reduced to a very low figure. The static element in the muscular effort has become dominant, and static expenditure is parasitic on dynamic work. The more static the work becomes the greater is the fall in the efficiency. Personally I am of the opinion that the severity or hardness of muscular work, *qua* the organism as a whole, is a function of the static component of the effort made. Fatigue, *i.e.* inability to carry on, is more readily induced by static work than by either

positive or negative work. The following figures from experiments which I have carried out with Miss Bedale and G. McCallum show clearly this diminution in efficiency as the static element in the work is increased:—

Table V.

Pulls per min.	Kgm. per min.	Cost in grm. cals. per kgm. p. sq. m.	Net Efficiency per cent.
32	40	16	8·0
12	15	17	7·5
6	7·5	20	6·0
4	5·0	31	3·0+
3	3·75	38	3·0
2	2·5	68	2·0—
1	1·25	146	1·0

Another series of experiments carried out with Burnett in another fashion led to the same conclusion.

Very closely allied with the rate of working is the rhythm with which the work is performed. Although they are not identical phenomena, they are so closely related that the habit of work may be considered along with rhythm. Sir Charles Sherrington and Graham Brown have both shown very definitely, in connection with their work on reciprocal innervation, that a rhythmic phenomenon may be evoked in muscle by the appropriate balance of antagonistic stimuli. Graham Brown holds that this rhythmic action is one of the most fundamental properties of the nervous system. Everyone is well aware that once a rhythm, or the proper co-ordination in the play of a set of muscles in the performance of some definite act, is mastered, not only is the energy expenditure reduced by the exclusion of numerous extraneous muscular activities, but there is an actual enhancement of the ease with which we perform the specified act. Willingly or unwillingly, those who have to do much repetitive work, be it playing golf, a musical instrument, or working a machine, soon appreciate the fact, when they think about it at all, that their best and easiest results are obtained under certain very definite conditions. To take a single example, the work of forward progression or walking is performed most easily when we adopt our own gait. It is not a mere question of rate. In a series of experiments which I carried out with Burnett, the subject, working on a specially geared ergometer, was allowed to select his own rate of working, the load being varied from nothing to 4 kilos. At each change of load the subject was directed either to work rapidly or very slowly, and after a period of such work was told to adopt the rate he liked best. As the following table (Table VI.) shows, the rhythm of work was practically identical for all loads. This occurred under all conditions, provided the working spells were not of too long duration. If the work were continued over a long period the rhythm tended to alter, to increase in speed, and if the subject became really tired, periods of rapid movement alternated with periods of slow movement.

Table VI.

Load in kilos.	Rate of Work per min. voluntarily selected			
	Exp. I.	Exp. II.	Exp. III.	Exp. IV. (Immediately after 1 hour's work at rate of 45 per min.)
0	78	80	83	—
1	80	79	79	71
2	81	80	81	—
3	80	78	83	73
4	82	77	78	—

The figures given are the averages of three or more observations made at each load. None of the observations were made in the order in which they are recorded; light and heavy loads were alternated.

This rhythm of work is simply a general example of the formation of a conditioned reflex. The rhythm adopted, although it may suit the worker, is not of necessity the series of muscle movements which lead to the least expenditure of energy. Most probably the rhythm selected is only in small part due to the worker's physical configuration; in greater part it is evolved in imitation of some more experienced or older worker. The average workman is not so much concerned with the diminution of the physiological cost in the performance of a given act as in the reduction of conscious effort. As Vernon states, 'Experienced industrial workers unconsciously adopt habits of work which tend to the production of a maximum output with the minimum of effort.'

This capacity of the organism to build up a series of conditioned reflexes is one of the potent factors in the prevention of fatigue. The organism is able not only to build up reflexes in response to the tactile impressions of the material which he handles, of the tools, their shape, weight, &c., with which he works, but even to the extent and duration of the movements which he develops in the performance of his work. The proper and effective linking up of a series of these stimuli lead to a technical rhythm which will not necessarily be identical in the case of each worker in the same shop performing the same operation, but which, viewed generally, will give a colourable representation of uniformity.

It is not, of course, suggested that the methods adopted by workers independently are the perfect methods, and that proper investigation will not discover better and easier methods of performing certain given operations. If newer and more economical methods are to be developed and brought into operation, the only real chance will be to segregate the newer young workers. Vernon gives an excellent example of the necessity of doing so. The output of a certain necessary stock article had to be increased. A factory concerned in the production turned out 5,000 per week, and this could not be increased—the regular workmen had a certain rate and habit of work. A new factory was started, staffed with hands new to the work, and after six months' practice they produced 13,000 articles per week.

There is good evidence, that of Muscio for example, that both resting and working, in addition to the individual muscle rhythm, there

is a definite variation in the course of the day in the capacity to carry out work; that, in other words, a diurnal rhythm exists. There is a certain amount of evidence also in favour of the view that a seasonal rhythm exists. As Wilson has put it, 'It is tempting to suppose that human performance may be dependent on a number of superimposed rhythms corresponding with the periods of work, beginning perhaps with the rhythm of the actual movement and ending with a seasonal rhythm.'

Further, when efficiency is measured in terms of output it is found that there is a definite rhythm in output during the course of the working day and of the working week. It is well known that it takes an appreciable time each day to work up to full power, and this is shown distinctly in the daily output curve, which rises and then drops sharply at the end of the work period. This type of curve is not peculiar to any one industry. The rise is almost certainly due to the 'limbering up,' and it is probable that the fall towards the end is largely due to voluntary slowing. The total weekly output curve with the low Monday effect and the sharp fall on Saturday resembles in general shape the daily output curve. The main point about these curves is that they seem to demonstrate the absence of progressive fatigue from overwork, which would have been deduced had there been a sharp rise at the commencement of the week, followed by a steady fall.

The third of the potent factors in the control of fatigue is rest. If work is done, rest is ultimately imperative. Rest not merely relaxes the muscles, allowing a more thorough and complete removal of the waste products and a more abundant supply of oxygen, but it removes the strain of attention. Rest is best obtained not by simple quiescence but by change of posture; slow movement of another type to that which produced the fatigue will, unless the organism is tired practically to complete exhaustion, give the most beneficial results. It is common knowledge that when the attention is concentrated, when our interest, either in the work or in something cognate or even foreign to it, is thoroughly aroused, spells of work or intensity of effort in its performance may be borne which under other circumstances might tax our resources to the last degree.

Forced work, *i.e.* work carried out at a pace other than that of the performer's own selection, is much more exhausting and destructive than where the subject is permitted to work at a rate of his own selection. Laulanié, assuming that fatigue had a purely physical origin, went the length of maintaining that muscles spontaneously find the optimum rate of work where the intervals of repose exactly suffice for sufficient recuperation, so that long spells of work may be done. Such a conclusion does gain a certain amount of support from the consideration, for example, of the cardiac cycle.

So far, little attention has been paid to the duration of the rest period in relation to the work done. As a general rule, it may be said that, in the majority of occupations, although the hours of labour are continuous, the actual spells of hard manual work are discontinuous, either due to the fact that certain operations are intermittent in their severity,

that supplies of material are not constant, or that, if these more or less natural conditions do not operate, rests at irregular intervals are deliberately taken by the operative.

So far as I am aware there is only one type of hard work where a definite rest period is laid down as part of the exercise, namely, in Army route marching. Marching as a costly form of energy expenditure is unique in that it is a continuous repetitive act carried on frequently for hours on end. The Regulations lay down that a definite rest period shall be taken every hour. Marching, too, is peculiar in another way, namely, that the rate of marching is fixed, *i.e.* a certain definite rate has to be constantly maintained throughout the marching period.

It can be shown that if a certain distance has to be covered in a definite time there is an optimum rate of forward progression. Cathcart, Lothian, and Greenwood found, for example, that when the cost per mile alone was considered, the minimum value was reached at a rate of about three miles per hour, but when the question of time as well as distance arose, *i.e.* if a distance of, let us say, one mile was to be covered and sixty minutes were available to do it in, would it be more economical to march the mile rapidly or slowly and have a longer or shorter period of rest? Our experiments showed that the lowest hourly value was obtained at a rate of a mile in about twenty-three minutes, *i.e.* marching for twenty-three minutes in the hour and resting for thirty-seven minutes. To spend thirty-two minutes on the march cost about 5 per cent. more, and to reduce the marching time to sixteen minutes increased the cost by about 15 per cent.

So much, then, for the ordinary effector factors. There are many other factors directly concerned with the efficient action of the organism, some directly influencing the internal economy of the body, others acting more indirectly on the organism from the environment.

One of these factors is the state of the nutrition. It may be definitely stated that an insufficient intake of food or the consumption of poor or inadequate food is one of the chief sources of general inefficiency. Our resistance to the effects of hard and continual work, just as to the effects of an infection, is largely controlled by our reserves. The capacity of the body to store reserve food material which will meet the daily demands for energy and leave a surplus is another of the vital factors of safety. The body is undoubtedly capable of withstanding complete deprivation of food for comparatively long periods, but with a corresponding depression in its capacity to perform external work. Complete starvation is a state which is rare, and does not affect the question at issue. The much more important problem is unfortunately only too common, the influence of chronic undernutrition, a condition which lowers efficiency, not merely in the actual performance of muscular work, but by inducing an increased susceptibility to disease. This is a question which has never received the attention which its importance demands, largely on account of the immense difficulties of carrying out the investigation in a practical manner. As the direct result of the war we have the records of at least two sets of observers. Benedict and his co-workers investigated the problem,

using a group of twelve men, comparing them with a similar group drawn from the same class. In the experimental group the food intake was reduced, so that there was a loss of 12 per cent. of the body weight. Although the experiment was carried on for over four months, and the basal metabolism was reduced by 18 per cent., the diminution in muscle power, so far as laboratory tests were concerned, was not great. The subjective impression, however, of the subjects was that they felt weaker and less capable.

The other recorded experiment is that of the condition in Germany during the war years. A general statement of the effects of the blockade is contained in a long document prepared by the German Government (dated December 1918). Admittedly the document was prepared for a specific purpose; but, after making all allowances, the record of the far-reaching effects of chronic underfeeding is valuable. It may be remarked that many of the statements receive corroboration in the report drawn up by Professor Starling on the food conditions in Germany. Apart from the increased death rate, the increased liability to disease, and the slow recovery from the attacks of disease, the document definitely states that the working capacity of the people was reduced by at least one-third. The following sentence gives probably an accurate picture of acute undernutrition, associated, it is true, with much emotional strain: 'Everywhere in Germany it may now be observed how, in the four years' struggle for daily bread, the people have lost all their vigour and capacity for work; how all spirit of enterprise is gone.'

Here, then, we have the actual record of an experiment on a gigantic scale. Personally I believe that these results are much more likely to be the ordinary sequence of underfeeding than the laboratory results of Benedict. I do not doubt for a moment his records, they are beyond reproach; but I feel that many of the excellent results which were obtained when the test subjects competed with subjects on ordinary food were in part due to the quite natural desire of the men to demonstrate to their friends and the onlookers that they were fit. In other words, there was a strong psychic element which vitiated the test as a real test of efficiency.

Evidence, much debated it is true, would indicate that it is not only the quantity but the quality of the food consumed which plays a part in the fitness of the individual to perform hard muscular work. All modern work would seem to point to the conclusion that if the caloric value of the food supplied is adequate the actual demand for protein is very small. It is very difficult, however, to believe that the far-reaching common belief in the efficacy of a high meat intake, despite the scientific evidence to the contrary, is without some foundation. It is possible that the value of meat (flesh) depends not merely on the high biological value of its protein, but also on the fact that it can act as a stimulant of cellular activity; that, in other words, it is desired for its stimulating effect, for giving, in that expressive transatlantic word, 'pep.'

Another factor which plays an enormous rôle in the general efficiency is the response of the organism to the multiple psychic imponderabilia

which compose such a large part of the average environment. Although for general purposes, such as the calculation of mean or average demands, it may be both convenient and useful to assess the data on an average man basis, yet it is the individual variation which controls the actual operating conditions. When we are dealing with the efficiency of the human organism, male and female, we are dealing with individuals whose performance is neither uniform throughout the year nor from week to week, nor even from hour to hour. We have to deal with an organism, as I have already mentioned, which is not only under physical control, but is very responsive to psychic influences—an organism which not only becomes in the course of the day physically 'tired,' but which unconsciously is influenced to an enormous extent by its environment, by its 'atmosphere.' Man is, in the main, a psychic chameleon.

In this connection monotony of work must be considered. The Health of Munition Workers' Committee stated that monotony is 'analogous to, if it does not represent, a fatigue process in unrecognised nerve centres.' It is true that monotonous work, be it light or heavy, may be, as Goldmark maintains, 'more damaging to the organism than heavier work which gives some chance of variety, some outlet for our innate revolt against unrelieved repetitions.' Still, although there may be a close relationship between monotony and fatigue, as generally recognised, they are not identical. The temperament of the operative plays an enormous part in the determining whether or no any particular operation is a monotonous one. Thus a skilled engineer put on to attend an automatic machine which requires the minimum of skilled attention would soon be disgusted with the monotony of the operation even if the pay were good, whereas an unskilled worker, especially if of a lower degree of intelligence, could attend to such a machine without strain. Munsterberg has recorded a number of most interesting examples of this seeming imperviousness to monotony even on the part of apparently intelligent individuals. Probably, indeed, if the operation is a slow one, or, if quick, one in which the movements are relatively simple, the dull individual—the organism with few trained receptors—and the worker with intelligence above the average, who readily masters the performance and can then let his mind free to speculate on his own private interests, will stand the strain better than the worker of average intelligence. As Munsterberg has shown, it is extremely difficult, if not impossible, for an outsider to determine what a monotonous operation is. It is obvious that if A is interested, let us say, in epigraphy, he will judge that B, engaged in some simple routine stamping or packing job, is employed on a monotonous operation, whereas it would be equally likely that if B were asked his opinion he would candidly state that he would not exchange his interesting work for the dull and monotonous pursuit of A.

There are many other factors which play a definite and important rôle in the maintenance of efficiency, such as lighting, heating, ventilation, the mode of life led by the worker outside his definite hours of labour, his housing, &c. Many of these factors have been partially examined. Thus Leonard Hill has carried out a great deal of valuable

work on the influence of the cooling power of the air. Vernon has collected much interesting evidence which shows that there is a very definite relation between the efficiency, as measured by output, and the temperature of the working place. The output in the hottest weather was about 30 per cent. below that when the weather was coldest. He also observed an apparent connection between the relative humidity of the air and the efficiency of the worker. The efficiency, as might have been expected, was apparently greatest when the relative humidity was low. Elton has reported on the influence of lighting in silk weaving. He found that the output was lowest when artificial light was used. He stated that even when electric light of sufficient intensity was used the output was about 10 per cent. below the daylight value. The actual equipment of the factories, the provision of seats of suitable size, height, &c., the design of the machines, and so on, all play their part, as is shown by the many records, particularly from the United States.

In other words, the real overall industrial efficiency of the worker cannot be causally related to any single factor. It is not the mere capacity of the individual to perform so many kilogrammètres of work in a given time with the smallest expenditure of energy. The quest of efficiency is one of the most intricate problems in its infinite ramifications throughout the physiological and sociological structure which has ever called for solution, and it involves the whole welfare of our race and nation. It calls for the closest and most intimate co-operation between the scientific investigator, the employer and the employee, and is no more capable of being settled on a communistic than on a capitalistic basis. It can only be satisfactorily attacked when mutual distrust of motives, capacities, and methods is stilled.

THE INFLUENCE OF THE LATE W. H. R. RIVERS

(PRESIDENT ELECT OF SECTION J)

ON THE DEVELOPMENT OF PSYCHOLOGY IN GREAT BRITAIN.

ADDRESS TO SECTION J (PSYCHOLOGY) BY

CHARLES S. MYERS, C.B.E., M.A., M.D., Sc.D., F.R.S.,

PRESIDENT OF THE SECTION.

A MOURNFUL gloom has been cast over the proceedings of our newly born Section. Since its inauguration twelve months ago this Section, as, indeed, Psychology in general, has suffered an irreparable loss through the sudden death, on June 4 last, of him who was to have presided here to-day. When, only a few weeks ago, it fell to me, as one of his first pupils, to occupy Rivers's place, I could think of little else than of him to whom I have owed so much for nearly thirty years of intimate friendship and invaluable advice; and I felt that it would be impossible for me then to prepare a Presidential Address to this Section on any other subject than on his life's work in psychology.

William Halse Rivers Rivers was born on March 12, 1864, at Luton, near Chatham, the eldest son of the Rev. H. F. Rivers, M.A., formerly of Trinity College, Cambridge, and afterwards vicar of St. Faith's, Maidstone, and of Elizabeth, his wife, *née* Hunt. Many of his father's family had been officers in the Navy—a fact responsible, doubtless, for Rivers's love of sea voyages. The father of his paternal grandfather, Lieutenant W. T. Rivers, R.N., was that brave Lieutenant William Rivers, R.N., who as a midshipman in the *Victory* at Trafalgar was severely wounded in the mouth and had his left leg shot away at the very beginning of the action, in defence of Nelson or in trying to avenge the latter's mortal wound. So at least runs the family tradition; also according to which Nelson's last words to his surgeon were: 'Take care of young Rivers.' A maternal uncle of Rivers was Dr. James Hunt, who in 1863 founded and was the first President of the Anthropological Society, a precursor of the Royal Anthropological Institute, and from 1863 to 1866 at the meetings of this Association strove to obtain that recognition for anthropology as a distinct Sub-section or Section which was successfully won for Psychology by his nephew, who presided over us at the Bournemouth meeting in 1919, when we were merely a Sub-section of Physiology.

Our 'young Rivers' gave his first lecture at the age of twelve, at a debating society of his father's pupils. Its subject was 'Monkeys.' He was educated first at a preparatory school at Brighton, and from

1877 to 1880 at Tonbridge School. Thence he had hoped to proceed to Cambridge; but a severe attack of enteric fever compelled him to take a year's rest, and thus prevented him from competing for an entrance scholarship at that University. He matriculated instead in the University of London, and entered St. Bartholomew's Hospital in 1882, sharing the intention of one of his father's pupils of becoming an Army doctor. This idea, however, he soon relinquished; but, like his desire to go to Cambridge, it was to be realised later in life.[1]

When he took his degree of Bachelor of Medicine in 1886 he was accounted the youngest Bachelor ever known at his hospital. Two years later he graduated as Doctor of Medicine, and he spent these two and the two following years in resident appointments at Chichester (1888) and at St. Bartholomew's (1889) hospitals, in a brief period of private medical practice (1890), and in travelling as ship's surgeon to America and Japan (1887), the first of numerous subsequent voyages. In 1891 he became house-physician at the National Hospital, Queen Square, where he first made the acquaintance of Dr. Henry Head, whose collaborator he was to be some twenty years later in one of the most striking neurological experiments ever made.

But before he began work at Queen Square, before he assisted Horsley there in his then wonderful operations on the brain, before he met Head fresh from his studies in Germany and enthusiastic over the colour-vision work and novel physiological conceptions of Hering, Rivers had already shown his interest in the study of the mind and the nervous system. Thus, in 1888, when he was twenty-four years of age, we find in the *St. Bartholomew's Hospital Reports* (vol. xxiv., pp. 249-251) his first published paper on 'A Case of Spasm of the Muscles of the Neck Causing Protrusion of the Head,' and in the following year, in the same *Reports* (vol. xxv., pp. 279-280), an abstract of a paper read by him before the Abernethian Society entitled 'Delirium and its Allied Conditions.' At this early date he pointed out the analogies between delirium and mania, protested against the use of narcotics in delirium, and condemned the wide separation—too wide even to-day—between diseases of the mind and diseases of the body. In 1891 and in 1893 he read papers to the Abernethian Society, abstracts of which appear in the *St. Bartholomew's Hospital Reports* (vol. xxvii., pp. 285-286, vol. xxix., p. 350), on 'Hysteria' and on 'Neurasthenia,' to which his interests were to return so fruitfully during and after the Great War.

In 1892 he spent the spring and early summer at Jena, attending the lectures of Eucken, Ziehen, Binswanger, and others. In a diary kept by him during this visit to Germany the following sentence occurs: 'I have during the last few weeks come to the conclusion that I should go in for insanity when I return to England and work as much as possible at psychology.' Accordingly, in the same year he became Clinical Assistant at the Bethlem Royal Hospital, and in 1893 he assisted G. H. Savage in his lectures on mental diseases at Guy's

[1] For many of the above details of Rivers's early life and antecedents I am indebted to his sister, Miss K. E. Rivers.

Hospital, laying special stress on their psychological aspect. About the same time, at the request of Professor Sully, he began to lecture on experimental psychology at University College, London.

Meanwhile, at Cambridge Michael Foster was seeking someone who would give instruction there in the physiology of the sense organs, McKendrick having, as Examiner in Physiology, recently complained of the inadequate training of the Cambridge students in this branch of the subject. Foster's choice fell on Rivers, and in 1893 he invited him to the University for this purpose. For a few months Rivers taught simultaneously at Cambridge and at Guy's Hospital and at University College, London. He went to Germany for a short period of study under Professor Kräpelin, then of Heidelberg, whose brilliant analysis of the work curve and careful investigations into the effects of drugs on bodily and mental work had aroused his intense interest. In collaboration with Kräpelin he carried out a brief investigation into mental fatigue and recovery, published in 1896 (*Journal of Mental Science*, vol. xlii., pp. 525-29, and Kräpelin's *Psychologische Arbeiten*, vol. i., pp. 627-78), which indicated that even an hour's rest is inadequate to neutralise the fatigue of half-an-hour's mental work, and paved the way for Rivers's important researches some ten years later upon the effects of drugs on muscular and mental fatigue.

At Cambridge Rivers set himself to plan one of the earliest systematic practical courses in experimental psychology in the world, certainly the first in this country. In 1897 he was officially recognised by the University, being elected to the newly established Lectureship in Physiological and Experimental Psychology. But the welcome and encouragement he received from cognate branches of study at Cambridge could hardly be called embarrassing. Even to-day practical work is not deemed essential for Cambridge honours candidates in elementary psychology; psychology is not admitted among the subjects of the Natural Sciences Tripos; and no provision is made for teaching the subject at Cambridge to medical students. Rivers first turned his attention principally to the study of colour vision and visual space perception. Between 1893 and 1901 he published experimental papers 'On Binocular Colour-mixture' (*Proc. Cambs. Philosoph. Soc.*, vol. viii., pp. 273-77), on 'The Photometry of Coloured Papers' (*J. of Physiol.*, vol. xxii., pp. 137-45), and 'On Erythropsia' (*Trans. Ophthal. Soc., London*, vol. xxi., pp. 296-305), and until 1908 he was immersed in the task of mastering the entire literature of past experimental work on vision, the outcome of which was published in 1900 as an article in the second volume of the important 'Text-book of Physiology' edited by Sir Edward Sharpey Schafer.

This exhaustive article of 123 pages on 'Vision' by Rivers is still regarded as the most accurate and careful account of the whole subject in the English language. It is of special value not only as an encyclopædic storehouse of references to the work of previous investigators—although with characteristic modesty Rivers omits to mention himself among them—but also for the unsurpassed critical account of the principal theories of colour vision. In it he displayed the strength and the weakness of Hering's theory and the untenability of Helmholtz's

explanations of successive contrast as due to fatigue, and of simultaneous contrast as due to psychological factors. Rivers clearly showed that the effect of psychological factors is not to create but to mask the phenomena of simultaneous contrast, which are really dependent on what he terms 'the physiological reciprocity of adjoining retinal areas.' His enthusiasm for Hering's theories led him to give by far the most detailed presentation of them that had then or has since appeared in our language. In classifying the phenomena of red-green colour-blindness, on which Helmholtz largely based his trichromic theory, Rivers proposed the useful terms 'scoterythrous' and 'photerythrous' in place of the terms 'protanopic' and 'deuteranopic,' so as to avoid, in describing these phenomena, the use of names which implied the acceptance of a particular theory of colour vision. These terms have failed, however, to obtain general adoption.

In 1896 Rivers published an important paper 'On the Apparent Size of Objects' (*Mind*, N.S., vol. v., pp. 71-80), in which he described his investigations into the effects of atropin and eserin on the size of seen objects. He distinguished two kinds of micropsia which had hitherto been confused—micropsia at the fixation-point due to irradiation, and micropsia beyond the fixation-point, which is of special psychological importance. Rivers came to the interesting conclusion that the mere effort to carry out a movement of accommodation may produce the same micropsia as when that effort is actually followed by movement. In other words, an illusion of size may be dependent solely on central factors. His later work, in conjunction with Professor Dawes Hicks, on 'The Illusion of Compared Horizontal and Vertical Lines,' which was published in 1908 (*Brit. J. of Psychol.*, vol. ii., pp. 241-60), led him to trace this illusion to origins still less motor in nature. Here horizontal and vertical lines were compared under tachistoscopic and under prolonged exposure. The momentary view of the lines in the tachistoscope precluded any movement or effort of movement of the eyes, which had been supposed by many to be responsible for the over-estimation of vertical lines owing to the greater difficulty of eye movement in the vertical as compared with the horizontal direction. The amount of the illusion was found to be approximately the same for tachistoscopic as for prolonged exposure of the lines, but in the tachistoscopic exposure the judgment was more definite and less hesitating—in other words, more naive, more purely sensory, more 'physiological'—than in prolonged exposure. This result, which led to further work by Dr. E. O. Lewis at Cambridge under Rivers upon the Müller-Lyer illusion and upon the comparison of 'filled' and 'empty' space, is of fundamental psychological importance. Although it is not inconsistent with the view that visual space perception depends *for its genesis* on eye movement, it compels us to admit that visual space perception, *once acquired*, can occur in the absence of eye movement; or, in more general language, that changes in consciousness, originally arising in connection with muscular activity, may later occur in the absence of that activity. The provision of experimental evidence in favour of so fundamental and wide-reaching a view is obviously of the greatest importance.

In 1898, in which year he was given the degree of Hon. M.A. at Cambridge, Rivers took a fresh path in his varied career by accepting Dr. A. C. Haddon's invitation to join the Cambridge Anthropological Expedition to the Torres Straits. This was the first expedition in which systematic work was carried out in the ethnological application of the methods and apparatus of experimental psychology. His former pupils, Prof. W. McDougall and I, assisted Rivers in this new field. Rivers interested himself especially in investigating the vision of the natives—their visual acuity, their colour vision, their colour nomenclature, and their susceptibility to certain visual geometric illusions. He continued to carry out psychological work of the same comparative ethnological character after his return from the Torres Straits in Scotland (where he and I sought comparative data), during a visit to Egypt in the winter of 1900, and from 1901-2 in his expedition to the Todas of Southern India.

The Torres Straits expedition marked a turning-point in Rivers's life interests, as they were for the first time directed towards ethnological studies, to which he became ardently devoted ever after, until his death removed one who at the time was President of the Royal Anthropological Institute, had in 1920-1 been President of the Folk Lore Society, and had in 1911 been President of Section H (Anthropology) of this Association. His ethnological and sociological work during his expedition to the Todas and during his two subsequent expeditions to Melanesia are too well known to need mention here. It was Rivers's own view that his most important contributions to science are to be found in the two volumes of his 'History of Melanesian Society,' published in 1914.

His psychological investigations among the Torres Straits islanders, Egyptians and Todas (*Reports of the Cambridge Anthrop. Exped. to Torres Straits*, vol. ii., Pt. I., pp. 1-132; *J. of Anthrop. Inst.*, vol. xxxi., pp. 229-47; *Brit. J. of Psychol.*, vol. i., pp. 321-96) will ever stand as models of precise, methodical observations in the field of ethnological psychology. Nowhere does he disclose more clearly the admirably scientific bent of his mind—his insistence on scientific procedure, his delight in scientific analysis, and his facility in adapting scientific methods to novel experimental conditions. He reached the conclusion that no substantial difference exists between the visual acuity of civilised and uncivilised peoples, and that the latter show a very definite diminution in sensibility to blue, which, as he suggested, is perhaps attributable to the higher macular pigmentation among coloured peoples. He observed a generally defective nomenclature for blue, green, and brown among primitive peoples, both white and coloured, and large differences in the frequency of colour-blindness among the different uncivilised peoples whom he examined. In his work on visual illusions he found that the vertical-horizontal-line illusion was more marked, while the Müller-Lyer illusion was less marked, among uncivilised than among civilised communities; and he concluded that the former illusion was therefore dependent rather on physiological, the latter rather on psychological factors, the former being contracted, the latter being favoured, by previous experience, *e.g.* of drawing lines or of apprehending complex figures as wholes.

In 1903, the year after his return from the Todas, and the year of his election to a Fellowship at St. John's College, Rivers began an investigation, continued for five years, with Dr. Henry Head, in which the latter, certain sensory nerves of whose arm had been experimentally divided, acted as subject, and Rivers acted as experimenter, applying various stimuli to the arm and recording the phenomena of returning cutaneous sensibility. The results of this heroic and lengthy investigation are well known. The discovery of a crude punctate protopathic sensibility, distinct from a more refined epicritic sensibility, so deeply impressed Rivers that a decade later his psychological views may be said to have been centred round this distinction between the ungraded, 'all-or-nothing,' diffusely localising functions of the protopathic system, and the delicately graded, discriminative, accurately localising functions of the epicritic system. The exact interpretation of this 'Human Experiment in Nerve Division,' published at length in 1908 (*Brain*, vol. xxxi., pp. 323-450), has been disputed by subsequent workers, whose divergent results, however, are at least partly due to their employment of different methods of procedure. Head's experiment has never been identically repeated, and until this has been done we are probably safe in trusting to the results reached by the imaginative genius and the cautious critical insight of this rare combination of investigators. At a far higher nervous level broad analogies to this peripheral analysis of cutaneous sensibility were later found by Head when thalamic came to be compared with cortical activity and sensibility.

While working with Head upon his arm Rivers's indomitable activity led him to simultaneous occupation in other fields. In 1904 he assisted Professor James Ward to found and to edit the *British Journal of Psychology*, and in that year he also received an invitation to deliver the Croonian Lectures in 1906 at the Royal College of Physicians, of which in 1899 he had been elected a Fellow. The study of drug effects had long interested him. In a paper on 'Experimental Psychology in Relation to Insanity,' read before the Medico-Psychological Society in 1895 (*Lancet*, vol. lxxiii., p. 867), he had drawn the attention of psychiatrists to the comparability of drug effects with the early stages of mental disorders before they were seen by the physician. And so, reverting to the work he had done under Kräpelin many years previously, he chose as his subject for the Croonian Lectures *The Influence of Alcohol and other Drugs on Fatigue* (Arnold, 1908). But although he utilised Kräpelin's ergograph and many of Kräpelin's methods, Rivers's *flair* for discovering previous 'faulty methods of investigation' and his devotion to scientific methods and accuracy could not fail to advance the subject. Of no one may it be more truly said than of him,—*nihil tetigit quod non ornavit*. He felt instinctively that many of the supposed effects of alcohol were really due to the suggestion, interest, excitement or sensory stimulation accompanying the taking of the drug. Accordingly he disguised the drug, and prepared a control mixture which was indistinguishable from it. On certain days the drug mixture was taken, on other days the control mixture was taken. the subject never knowing which he was drinking. Rivers engaged Mr. H. N. Webber as a subject who could devote himself to the investi-

gation so completely as to lead the necessarily uniform life while it was being carried out. He found that the sudden cessation of all tea and coffee necessary for the study of the effects of caffeine induced a loss of energy, and that other mental disturbance might occur through giving up all forms of alcoholic drink. Therefore most of his experiments were carried out more than twelve months after the taking of these drinks had been discontinued. Instead of recording a single ergogram Rivers took several sets of ergograms each day, each set consisting usually of six ergograms taken at intervals of two minutes, and separated from the next set by an interval of thirty or sixty minutes. He arranged that the drug mixture or the control mixture should be taken after obtaining the first set of ergograms, which served as a standard wherewith subsequent sets on the same day might be compared. He worked with Mr. Webber on alcohol and caffeine, and was followed by the similar work of Dr. P. C. V. Jones in 1908 on strychnine, and of Dr. J. G. Slade in 1909 on Liebig extract.

With these vast improvements in method Rivers failed to confirm the conclusions of nearly all earlier investigators on the effects of from 5 to 20 c.c. of absolute alcohol on muscular work. His results with these doses, alike for muscular and mental work, were mainly negative, and indeed with larger doses (40 c.c.) were variable and inconclusive; although an equivalent quantity of whisky gave an immediate increase of muscular work—a result which strongly suggests the influence of sensory stimulation rather than the direct effect of the drug on the central nervous system or on the muscular tissues. Rivers concluded that alcohol may in some conditions favourably act on muscular work by increasing pleasurable emotion and by dulling sensations of fatigue, but that probably its most important effect is to depress higher control, thus tending to increase muscular and to diminish mental efficiency. Working with caffeine, Rivers also obtained effects much less pronounced than those recorded by several earlier observers. He adduced evidence to indicate that (like alcohol) caffeine has a double action on muscular activity, the one immediately increasing the *height* of the contractions obtained and persisting, the other producing an initial slow, transitory increase in the *number* of the contractions, and then a fall. Following Kräpelin, he suggested that the former action represents a peripheral, the latter a central effect.

He also put forward novel suggestions as to the true course of the fatigue curve, and laid stress on the importance of carrying out ergographic work by peripheral electrical stimulation. These views are certain to bear fruit in the future. Indeed, it may be safely said that no one can henceforth afford to investigate the effect of drugs on the intact organism without first mastering Rivers's work on the subject.

From the concluding passages of these Croonian lectures the following sentences may be aptly cited: 'The branch of psychology in which I am chiefly interested is that to which the name of individual psychology is usually given. It is that branch of psychology which deals with the differences in the mental constitutions of different peoples, and by an extension of the term to the differences which characterise the members of different races. . . . These experiments leave little doubt

that variations in the actions of drugs on different persons may have their basis in deep-seated physiological variations, and I believe that the study of these variations of susceptibility may do more than perhaps any other line of work to enable us to understand the nature of temperament and the relation between the mental and physical characters which form its two aspects.'

Rivers's interests did not lie in the collection of masses of heterogeneous data, in obtaining blurred averages from vast numbers of individuals, in concocting mathematical devices, or in applying mathematical formulæ to the numerical data thus accumulated; they lay throughout his varied career in studying and analysing individual mental differences, in getting to know the individual in his relation to his environment. In ordinary circumstances, as he later said, 'There is too little scope for the variations of conditions which is the essence of experiment. . . . While the experimental method as applied to the normal adult has borne little fruit, it would be difficult to rate too highly the importance of experiment in discovering and testing methods to be used in other lines of psychological inquiry where a wider variation of conditions is present' (*Brit. J. of Psychol.*, vol. x., p. 185).

It was the importance of studying the play of the most variable conditions that led Rivers to investigate, as we have seen, first racial mental differences, then the differences produced in a given individual by nerve section, and finally those produced in different individuals by different drugs. Throughout his life he was steadfast to the biological standpoint, correlating the psychological with the physiological, and hoping to discover different mental levels corresponding to different neural levels.

And so we approach the last phase of Rivers's psychological work, the outcome of his war experiences. In 1907 he had given up his University teaching in experimental psychology; for six years before the war he had published nothing of psychological or physiological interest. This was a period in which Rivers devoted himself wholly to the ethnology and sociology of primitive peoples. The outbreak of war found him for the second time visiting Melanesia for ethnological field work. Failing at first to get war work on his return to England, Rivers set himself to prepare the Fitzpatrick Lectures on 'Medicine, Magic and Religion,' which he had been invited to deliver to the Royal College of Physicians of London in 1915 and 1916. In 1915 his psychological and ethnological researches were recognised by the award to him of a Royal Medal by the Royal Society, of which he had been elected a Fellow in 1908. In July 1915 he went as medical officer to the Maghull War Hospital, near Liverpool, and in 1916 to the Craiglockhart War Hospital, Edinburgh, receiving a commission in the R.A.M.C. In these hospitals he began the work on the psychoneuroses that led him to his studies of the unconscious and of dreams, which resulted in his well-known book, 'Instinct and the Unconscious,' published first in 1920 (already in a second edition), and in a practically completed volume on 'Conflict and Dream,' which is to be published posthumously. From 1917 he acted as consulting psychologist to the Royal Air Force, being attached to the Central Hospital at Hampstead.

This period not merely marks a new phase in Rivers's work, but is also characterised by a distinct change in his personality and writings. In entering the Army and in investigating the psychoneuroses he was fulfilling the desires of his youth. Whether through the realisation of such long-discarded or suppressed wishes, or through other causes, *e.g.* the gratified desire of an opportunity for more sympathetic insight into the mental life of his fellows, he became another and a far happier man. Diffidence gave place to confidence, hesitation to certainty, reticence to outspokenness, a somewhat laboured literary style to one remarkable for its ease and charm. Over forty publications can be traced to these years, between 1916 and the date of his death. It was a period in which his genius was released from its former shackles, in which intuition was less controlled by intellectual doubt, in which inspiration brought with it the usual accompaniment of emotional conviction—even an occasional impatience with those who failed to accept his point of view. But his honest, generous character remained unchanged to the last. Ever willing to devote himself unsparingly to a cause he believed right, or to give of his best to help a fellow-being in mental distress, he worked with an indomitable self-denying energy, won the gratitude and affection of numberless nerve-shattered soldier-patients, whom he treated with unsurpassed judgment and success, and attracted all kinds of people to this new aspect of psychology. Painters, poets, authors, artisans, all came to recognise the value of his work, to seek, to win, and to appreciate his sympathy and his friendship. It was characteristic of his thoroughness that while attached to the Royal Air Force he took numerous flights, 'looping the loop' and performing other trying evolutions in the air, so that he might gain adequate experience of flying and be able to treat his patients and to test candidates satisfactorily. He had the courage to defend much of Freud's new teaching at a time when it was carelessly condemned *in toto* by those in authority who were too ignorant or too incompetent to form any just opinion of its undoubted merits and undoubted defects. He was prepared to admit the importance of the conflict of social factors with the sexual instincts in certain psychoneuroses of civil life, but in the psychoneuroses of warfare and of occupations like mining he believed that the conflicting instincts were not sexual, but were the danger instincts, related to the instinct of self-preservation.

Thus in the best sense of the term Rivers became a man of the world and no longer a man of the laboratory and of the study. He found time to serve on the Medical Research Council's Air Medical Investigation Committee, on its Mental Disorders Committee, on its Miners' Nystagmus Committee, and on the Psychological Committee of its Industrial Fatigue Research Board. He served on a committee, of ecclesiastical complexion, appointed to inquire into the new psychotherapy, and he had many close friends among the missionaries, to whom he gave and from whom he received assistance in the social and ethnological side of their work.

In 1919, in which year he received honorary degrees from the Universities of St. Andrews and Manchester, he returned to Cambridge

as Prælector in Natural Sciences at St. John's College, and began immediately to exercise a wonderful influence over the younger members of the University by his fascinating lectures, his 'Sunday evenings,' and above all by his ever-ready interest and sympathy. As he himself wrote, after the war work 'which brought me into contact with the real problems of life . . . I felt that it was impossible for me to return to my life of detachment.' And when a few months before his death he was invited by the Labour Party to a still more public sphere of work, viz., to become a Parliamentary candidate representing the University of London, once again he gave himself unsparingly. He wrote at the time: 'To one whose life has been passed in scientific research and education the prospect of entering practical politics can be no light matter. But the times are so ominous, the outlook both for our own country and the world so black, that if others think I can be of service in political life I cannot refuse.' On several occasions subsequently he addressed interested London audiences, consisting largely of his supporters, on the relations between Psychology and Politics. It was one of these very lectures—on the Herd Instinct—at which it happened that I took the chair, which was to have formed the basis of his Presidential Address to you here to-day.

Rivers's views on the so-called herd instinct were the natural outcome of those which he had put forward during the preceding five years and collected together in his 'Instinct and the Unconscious.' His aim in writing this book was, as he says, 'to provide a biological theory for the psychoneuroses,' to view the psychological from the physiological standpoint. He maintained that an exact correspondence holds between the inhibition of the physiologist and the repression of the psychologist. He regarded mental disorders as mainly dependent on the coming to the surface of older activities which had been previously controlled or suppressed by the later products of evolution. Here Rivers went beyond adopting Hughlings Jackson's celebrated explanation of the phenomena of nervous diseases as arising largely from the release of lower-level activities from higher-level controls. He further supposed that these lower-level activities represent earlier racial activities held more or less in abeyance by activities later acquired. This conception he derived from his work with Henry Head on cutaneous sensibility. Rivers could see but 'two chief possibilities' of interpreting the phenomena disclosed in the study of Head's arm. Either epicritic sensibility is protopathic sensibility in greater perfection, or else protopathic sensibility and epicritic sensibility represent two distinct stages in the development of the nervous system. Failing to see any other explanation, he adopted the second of these alternatives. He supposed that at some period of evolution, when epicritic sensibility, with its generally surface distribution, its high degree of discrimination, and its power of accurate localisation, made its appearance, the previously existing protopathic sensibility, with its punctate distribution, its 'all-or-nothing' character, and its broad radiating localisation, became in part inhibited or 'suppressed,' in part blended or 'fused' with the newly acquired sensibility so as to form a useful product. He supposed that the suppressed portion persisted in a condition of

unconscious existence, and he emphasised the biological importance of suppression. He considered at first that the protopathic sensibility 'has all the characters we associate with instinct,' whereas the later epicritic sensibility has the characters of intelligence or reason. So he came to hold that instinct 'led the animal kingdom a certain distance in the line of progress,' whereupon 'a new development began on different lines,' 'starting a new path, developing a new mechanism which utilised such portions of the old as suited its purpose.'

Evolutio per saltus was thus the keynote of Rivers's views on mental development. Just as the experience of the caterpillar or tadpole is for the most part suppressed in the experience of the butterfly or frog, so instinctive reactions tend to be suppressed in intelligent experience whenever the immediate and unmodifiable nature of the one becomes incompatible with the diametrically opposite characters of the other. Just as parts of the protopathic fuse with the later acquired epicritic sensibility, so parts of our early experience, of which other parts are suppressed, fuse with later experience in affecting adult character. 'Experience,' he explained, 'becomes unconscious because instinct and intelligence run on different lines and are in many respects incompatible with one another.'

Rivers was compelled later to recognise 'epicritic' characters in certain instincts. He came to suppose that 'the instincts connected with the needs of the individual' and with the early preservation of the race are mainly 'of the protopathic kind,' whereas the epicritic group of instincts first appeared with the development of gregarious life. He recognised the epicritic form of mental activity in the instincts connected with the social life, especially of insects, and also in the states of hypnosis and sleep. Finally, he doubted the validity of the usual distinctions between instinct and intelligence.

Throughout his work on this wide subject Rivers endeavoured to give a strict definition to words which had hitherto been ambiguously or loosely used. He defined *unconscious experience* as that which is incapable of being brought into the field of consciousness save under such special conditions as 'sleep, hypnosis, the method of free association and certain pathological states.' He defined *repression* as the self-active, 'witting' expulsion of experience from consciousness, and *suppression* as the 'unwitting' process by which experience becomes unconscious. Thus suppression may occur without repression. When one refuses to consider an alternative path of action, one represses it; when a memory becomes 'of itself' inaccessible to recall, it is suppressed. When such a suppressed experience acquires an independent activity which carries with it an independent consciousness, it undergoes, according to Rivers's usage of the term, *dissociation*. Thus suppression may occur without dissociation. In its most perfect form, according to Rivers, suppression is illustrated by the instinct of immobility which forms one of the reactions to danger; the fugue (as also somnambulism) is 'a typical and characteristic instance of dissociation.'

From his point of view Rivers was naturally led, wherever possible, to interpret abnormal mental conditions in terms of regression to more

primitive, hitherto suppressed activities. He held that the hysterias are essentially ' substitution neuroses,' connected with and modified by the gregarious instincts, and are primarily due to a regression to the primitive instinctive danger reaction of immobility, greatly modified by suggestion. So, too, he held that the anxiety neuroses, which are for him essentially ' repression neuroses,' also show regression, though less complete, in the strength and frequency of emotional reaction, in the failure during states of phantasy to appreciate reality, in the reversion to the nightmares, and especially the terrifying animal dreams, characteristic of childhood, in the occurrence of compulsory acts, in the desire for solitude, &c. Indeed, because he believed that suppression is especially apt to occur, and to be relatively or absolutely perfect, in infancy, Rivers suggested that the independent activity of suppressed experience and the process of dissociation, as exemplified in fugues, complexes, &c., are themselves examples of regression.

He criticised Freud's conception of the censorship, substituting in place of that anthropomorphically-coloured sociological parallel the physiological and non-teleological conception of regression. He supposed the mimetic, fantastic, and symbolic forms in which hysterias and dreams manifest themselves to be natural to the infantile stages of human development, individual or collective. For him they were examples of regression to low-level characters, and not, as Freud supposes, ascribable to compromise formations to elude the vigilance of an all-protective censor. He regarded nightmares and war-dreams as examples of infantile states. He believed the absence of affect in many normal dreams to be natural to the infantile attitude, which would treat the situation in question with indifference. That absence of affect also arises from the harmless symbolic solution of the conflict. The affect of dreams is only painful, Rivers supposed, when they fail to provide a solution of the conflict, and is not due, as Freud holds, to the activity of the censor. In the social behaviour of primitive communities Rivers was able to find striking analogies to the characteristics of dreams, as described by Freud.

On the protopathic side he ranged the primitive instincts and emotions, and the complexes, together with the activities of the optic thalamus, and on the epicritic side intelligence and the sentiments, together with the activities of the cerebral cortex. We are now in a position to examine Rivers's treatment of the gregarious behaviour of animal and human life, on which he was still engaged at the time of his death. In the gregarious instinct he recognised a cognitive aspect which he termed ' intuition,' an affective aspect which he termed ' sympathy,' and a motor aspect which he termed ' mimesis.' He used ' mimesis ' for the process of imitation so far as it was unwitting, ' Sympathy ' he regarded as always unwitting. ' Intuition ' he defined as the process whereby one person is unwittingly influenced by another's cognitive activity. But I feel sure that the term ' unwittingly ' is not to be considered here as equivalent to ' telepathically.' All that Rivers meant was that the person is influenced by certain stimuli without appreciating their nature and meaning. He preferred to employ the term ' suggestion ' as covering

all the processes by which one mind acts on or is acted on by another unwittingly. He supposed that in the course of mental evolution epicritic characters displaced the early protopathic characters of instinctive behaviour owing to the incidence of gregarious life, especially among insects, and owing to the appearance and development of intelligence, especially in man. The suggestion inherent in gregarious behaviour implies some graduation of mental and bodily activity —an instinctive and unwitting discrimination distinct from the witting discrimination of intelligence. Suggestion, in primitive gregarious behaviour, as also in the dissociated state of hypnosis, and in its allied form, ordinary sleep, is prevented if witting processes be active; it 'is a process of the unconscious,' said Rivers. Both within the herd and during hypnosis, which he believed to be fundamentally of a collective nature, sensibility is heightened, so that the organism may be able to react to minute and almost imperceptible stimuli. Were he here to-day Rivers would have carried this conception of the evolution of gregarious life still further by distinguishing between the more lowly leaderless herd and the herd which has acquired a definite leader. He would have traced the development of the new affect of submission and of the new behaviour of obedience to the leader, and he would doubtless have accredited the leader with the higher affects of superiority and felt prestige, with the higher cognition that comes of intuitive foresight, and with the higher behaviour of intuitive adaptation, initiative, and command. I expect, too, that he would have sketched the development of still later forms of social activity, complicated by the interaction and combination of intellectual and instinctive processes —the witting deliberations and decisions on the part of the leader, and the intellectual understanding of the reasons for their confidence in him and for their appropriate behaviour on the part of those who are led.

But it would be idle further to speculate on the ideas of which we have been robbed by Rivers's untimely death. Let us rather console ourselves with the vast amount of valuable and suggestive material which he has left behind and with the stimulating memories of one who, despite the fact that his health was never robust, devoted himself unsparingly to scientific work and to the claims of any deserving human beings or of any deserving humane cause that were made upon him. There are, no doubt, some who believe that Rivers's earlier experimental psychological work—on vision, on the effects of drugs, and on cutaneous sensibility—is likely to be more lasting than his later speculations on the nature of instinct, the unconscious, dreams, and the psychoneuroses. No one can doubt the scientific permanence of his investigations in the laboratory or in the field; they are a standing monument to us of thoroughness and accuracy combined with criticism and genius. But even those who hesitate to suppose that at some definite period in mental evolution intelligence suddenly made its appearance and was grafted on to instinct, or that epicritic sensibility was suddenly added to a mental life which had before enjoyed only protopathic sensibility—even those who may not see eye to eye with Rivers on these and other fundamental views on which much of his later work rested, will be foremost in recognising the extraordinarily stimulating, suggestive, and fruitful

character of all that he poured forth with such astounding speed and profusion during the closing years of his life. And above all we mourn a teacher who was not merely a man of science devoted to abstract problems, but who realised the value of and took a keen delight in applying the knowledge gained in his special subject to more real and living problems of a more concrete, practical, everyday character. Rivers's careful methods of investigating cutaneous sensibility and the *rationale* of his successful treatment of the psychoneuroses were directly due to his psychological training. So, too, his epoch-making discoveries and his views in the field of anthropology on the spread and conflict of cultures were largely due to the application of that training. Shortly before his death he was developing, as a committee member of the Industrial Fatigue Research Board, an intense interest in that youngest application of psychology, viz., to the improvement of human conditions in industrial and commercial work by the methods of experimental psychology applied to fatigue study, motion study, and vocational selection.

Unhappily, men of such wide sympathies and understanding as Rivers, combined with a devotion to scientific work, are rare. He himself recognised that 'specialisation has . . . in recent years reached such a pitch that it has become a serious evil. There is even a tendency,' he rightly said, 'to regard with suspicion one who betrays the possession of knowledge or attainments outside a narrow circle of interests' (*Brit. J. of Psychol.*, vol. x., p. 184). Let his life, his wisdom, his wide interests, sympathies and attainments, and the generosity and honesty of his character, be an example to us in the common object of our meeting—the Advancement of Science.

TRANSPORT OF ORGANIC SUBSTANCES IN PLANTS.

ADDRESS TO SECTION K (BOTANY) BY

PROFESSOR H. H. DIXON, Sc.D., F.R.S.,

PRESIDENT OF THE SECTION.

PLANT physiologists have not paid the attention to the transport of organic substances which the importance of the subject appears to deserve. The ascent of water in high trees in defiance of gravity strikes the casual observer and forces him to speculation as to how it is contrived; but the problem of the transmission of organic substances throughout plants only forces itself on those who are more or less conversant with some of the leading facts of vegetable physiology.

Among physiologists the usually accepted view is that organic substances are distributed throughout the plant by means of the bast. The wood also acts as a channel of distribution for these substances to opening buds and developing leaves, especially in spring when root-pressure is active. The sap of bleeding contains appreciable quantities of these substances, and their distribution to the developing buds in spring by means of the wood was recognised by Hartig and Sachs. Fischer showed that even in summer many woody plants contain reducing sugars in their wood. The sugar content of the wood is at a maximum in spring, diminishes in summer, and is at a minimum in winter; at the end of February it rises rapidly. There is a second crest on the curve at leaf-fall Dr. Atkins and myself extended Fischer's observations, and showed that appreciable quantities of soluble carbohydrates, sucrose, hexoses, and even maltose, are found in the tracheæ of the stems and roots of many trees at all seasons of the year. Being in the tracheæ they must be carried from the lower parts to the upper growing regions, including the cambium of the stem. Samples drawn from different levels during the spring were found to have a greater concentration at higher levels. The inflow of water below, and consequent dilution, is probably largely responsible for this difference. Towards the end of the season there is no marked difference.

This upward transport of carbohydrates in the tracheæ seems to be accompanied with smaller amounts of proteins. Thus Schroeder showed that the quantity of proteins in the bleeding sap rises and falls with the quantity of sugar.

This view that the rising current in the tracheæ carries organic substances in it and distributes them to the growing regions has lately been impugned. It was pointed out that in many cases ringing close below the terminal bud prevents the development of that bud because

the wood is unable to transmit sufficient supplies of organic substance. As Strasburger has already pointed out, this interpretation rests upon the fallacy of supposing that the removal of the bark as far as the cambium leaves the wood uninjured. As a matter of fact, microscopic examination of the wood, from which the outer tissues have been stripped, shows that its tracheæ soon become blocked with air-bubbles and with substances probably exuded into them and their walls during morbid changes in the cells of the cambium, in the cells of the medullary rays, and in those of the wood-parenchyma. The blocking is accompanied with discoloration, and is most apparent in the outer layers of the wood. It is only reasonable to suppose that the efficiency of the tracheæ as channels of transmission is seriously impaired even before there is visible evidence of plugging.

It is evident that this clogging may act differentially on the water and the substances it carries in it. In the first place, the whole cross-section of the wood is available for the transport of water, while probably the outer layers are mainly utilised by the organic substances. Further, colloidal deposits in the walls, and especially in the pit-membranes, would obstruct the passage of organic substances much more than they would the water which carries them. These considerations readily explain how it is that, while the water-supply to the buds of ringed branches is adequate, the supply of organic substance may be deficient.

Apart, then, from the very slow movement of organic substances from cell to cell, there is very cogent evidence that their upward motion is effected in the tracheæ of the wood. There is no reason to believe that during this transport the walls or pit-membranes of these tracheæ oppose the passage of the dissolved carbohydrates or of the simpler proteins any more than the water which conveys them. Hence the velocity of transport of these organic substances is that of the transpiration current, and the amount conveyed in a given time depends on the velocity and concentration of the stream.

The transport of organic substances in an upward direction in plants is secondary, for, as is well known, carbohydrates certainly, and proteins most probably, are manufactured only in the upper green parts of plants—principally in the leaves, and must be transported in the first instance back from these to the stems to be distributed to the growing regions and to the storage organs.

It is almost universally held that the channel for this backward or downward motion of organic substances from the assimilating organs is the bast of the conducting tracts. The orthodox position is summed up by Strasburger as follows: In woody plants the carbohydrates manufactured by the leaves pass downwards in the bast. Movements of carbohydrates in the opposite direction in this tissue only take place if they are occasioned by local consumption. From the bast the carbohydrates spread into the medullary rays and wood-parenchyma, and in young branches fill these tissues and more or less of the pith. A downward movement in the wood-parenchyma, such as was formerly held, does not take place, and in those conifers in which there is no continuous wood-parenchyma is anatomically impossible. In spring and

also during summer growth an upward transport of carbohydrates in the water tracts occurs.

This view that the channel for the backward and downward movement of organic substances is afforded by the bast received great support from Czapek's work published in 1897. By section of the conducting tracts in one half of the petiole he showed that depletion of the corresponding half of the blade was delayed. He also showed that only where vertical bridges connected the upper and lower portions of bark in ringed stems were the effects of ringing nullified. Oblique and zigzag bridges are ineffective. Thus transverse conveyance in the stem is negligible. The parallel and longitudinal arrangement of the elongated elements in the bast seemed to him to provide adequately for the observed longitudinal passage. Their narrowness and large colloid content did not present themselves as difficulties.

He also recorded the observation that the blades of leaves, the petioles of which had been killed by jacketing them with steam, did not become emptied of starch. Similarly, when the petioles were killed with chloroform-vapour, depletion was arrested. Again, anæsthetisation of the petiole, by surrounding it with a watery solution of chloroform, greatly delayed the disappearance of starch. On the other hand, depletion was not obstructed when the petiole was immersed in a 5 per cent. solution of potassium nitrate. From this last observation he concluded that plasmolysis of the translocating elements does not interfere with their function as channels of transport.

Czapek formed no definite theory as to how organic substances were moved in the bast. He was sure that the transport depends on living protoplasm. He did not consider that the streaming of protoplasm contributed materially to the motion, seeing that streaming does not occur in mature sieve-tubes. He regarded the sieve-tubes as the most important elements in the transmission of these substances, because the deposition of callus in the sieve-plates synchronises with the stoppage of transport. The transport, according to him, is not simply due to diffusion. He supposed the protoplasm to take up the organic substances and pass them on. If diffusion does not account for the passage from one particle of protoplasm to the next, it would seem that we must suppose the organic substance to be projected from one to the other.

These observations and their interpretation by Czapek have strengthened the opinion that the bast is the channel for the downward transport of organic substances. It is remarkable how little weight has been attached to the damaging criticism of Czapek's views by Deleano, especially as those views are so unsatisfactory from a physical standpoint.

The latter author showed that it is inadmissible to compare externally similar leaves, which often behave, so far as depletion is concerned, very dissimilarly. He also pointed out that without any export a leaf may be depleted of all its starch within thirty-five hours, and partially anticipated an extremely interesting recent observation of Molisch—namely, that transpiring leaves lose their carbohydrates much more rapidly than those whose transpiration is checked by being sur-

rounded with a saturated atmosphere. Neglect of these facts led Czapek into error. Deleano also showed that organic substances continue to leave the blades even after the petioles have been killed by heat or by chloroform-vapour. The rate of depletion is reduced by the former agent to about one-third, and by the latter to one-half. If this observation is substantiated it would show that the intervention of living elements is not essential for the transport. He further found that the blades attached to petioles which were surrounded by chloroform-water lost their starch more quickly than those immersed in water.

The contradictory conclusions of Czapek and Deleano urgently call for a reinvestigation of the points at issue. It is not enough to assume, as Schroeder does, a completely sceptical attitude towards the latter investigator's account of his experiments. If Czapek's work holds good, we shall have to regard the bast, and especially the sieve-tubes, as the channels for the transport of organic substances back from the leaf-blades where they are manufactured, and we must look for some hitherto undreamed-of method of transmission through these most unlikely-looking conduits. On the other hand, if Deleano's conclusions are borne out, we should admit that protoplasm is not necessary for the transport, and we shall turn to a dead tissue as furnishing this channel.

So far as I am aware none of the earlier investigators made any estimate either of the actual quantities of organic material which are transported or of the velocities of flow in the channels which are necessary to effect this transport.

We may approach this problem from two opposite directions—(1) by dealing with the amount of organic substance accumulated in a given time in a storage organ, or (2) by using the amount exported from an assimilating organ. The cross-section of the supposed channels of transport and the volume of the solution containing the substances in each case will give us the other necessary data.

For the first method a potato-tuber will furnish an example. One weighing 210g. was found attached to the base of a plant by a slender branch about 0.16cm. in diameter. In this branch the bast had a total cross-section of 0.0042cm.^{2}. This figure is a maximum; no allowance was made for the cross-section of the cell-walls, or for any non-functional elements in the bast. The cell-walls would occupy probably one-fifth of the cross-section of the bast. Now if the bast exclusively furnished the channel of downward transport, all the organic substance in the potato must have passed this cross-section during the time occupied in the growth of the potato. One hundred days would be a liberal allowance. According to analyses more than 24 per cent. by weight of the potato is combustible. Therefore we must assume that during this time more than 50g. of carbohydrate has passed down a conduit having a cross-section of no more than 0.0042cm.^{2}. The average concentration of the solution carrying this substance could scarcely have been as much as 10 per cent. (2.5-5 per cent. would be more probable. The concentration of sugar in bleeding sap is much below this figure, and seems never to reach 4 per cent.). Assuming, however, this concentration, the volume of liquid conveying 50g. must have been 500cm.^{3}, and this quantity must

have passed in 100 days. Therefore the average velocity of flow through this conduit, having a cross-section of 0.0042cm.^{2}, must have been $\frac{500}{0.0042 \times 100 \times 24}$, *i.e.* nearly 50cm. per hour.

By the second method we arrive at a different figure. Various investigators, from Sachs onwards, have measured the rate of photosynthesis per square metre of leaf per hour. Under the most favourable conditions the amount may approach 2g., and it has been estimated as low as 0.5g. Taking Brown and Morris' determination for *Tropæolum majus*, viz., 1g. per square metre per hour, and assuming one-third of the carbohydrate formed is used in respiration in the leaf, we find that a leaf of 46cm.^{2} may form during ten hours' sunshine 0.46g.; during the twenty-four hours one-third of this will be respired, leaving 0.31g. to be transported from the leaf. The volume of the solution (again assuming a concentration of 10 per cent.) will be 3.10cm.^{3}. The cross-section of the bast of the bundles in the petiole was 0.0009cm.^{2}, therefore the velocity of flow, if the bast was used as the channel of transport, must have been $\frac{3.10}{0{\cdot}0009 \times 24}$ or 140cm. per hour.

Similar figures to these were derived using measurements obtained from a number of potato-tubers and from various leaves. The velocities indicated, even assuming a concentration of 10 per cent., lay in all cases between 20cm. and 140cm. per hour. These figures are in agreement with those arrived at by Luise Birch-Hirschfeld, as to the weight of organic material transported from leaves.

A flow of this rate through the bast seems quite impossible. The narrow transverse section of its elements, the frequent occurrence of transverse walls, and the lining of protoplasm and large protein contents practically preclude the mass movement of liquid through this tissue. If we imagine the flow restricted to the sieve-tubes the velocity must be correspondingly increased, and the excessively fine sieve-pores, more or less completely occupied by colloidal proteins, must be reckoned with. Simple diffusion, as Czapek recognised, cannot account for the transport, and there is no reason to suppose that adsorption on the surfaces of the colloid contents of the sieve-tubes can increase the velocity of diffusion, as Manghan suggests.

As soon as one realises the volume of the solution which has to be transported, and the velocity of the flow that this necessitates, one naturally turns to consider if the open capillary tubes of the wood may not be utilised as channels of transport. Deleano's results, indicating that the depletion of leaves continues even after the living elements of their petioles have been killed, support this conjecture.

The emphasis which has been laid on the function of the wood as providing a channel for the upward movement of water usually obscures its function as a downward and backward channel also. Early experimenters, however, fully recognised that, under certain conditions, the current in the wood may be reversed. Thus Hales quotes several experiments of his own proving this point, and states that some of his

were but repetitions of Perault's earlier work. In these experiments Hales showed that water applied at the top of a branch, which is severed from the tree, is drawn back into the branch and supplies its leaves, and makes good their transpiration losses. A tree inarched into two adjacent trees may continue to grow even after its connection with the ground is severed, drawing its supplies from its neighbours. Finally, a forked branch removed from a tree will remain fresh, and continue to transpire water for many days if one limb of the fork with its leaves is immersed in water. There is, of course, recent work also showing this reversed current.

By means of an eosin solution this reversal of the transpiration current may be very easily demonstrated. If the tip of a leaf of a growing potato-plant is cut under eosin solution, the coloured solution is very quickly drawn back into the tracheæ of the conducting tracts of the leaf; from there it passes into those of the petiole, and makes its way not only into the upper branches and leaves, but also passing down the supporting stem may completely inject the tracheæ of the tuber, and from thence pass up into the wood of the remaining haulms of the plant. Its passage is entirely in the tracheæ of the wood of the conducting tracts.

Another very striking experiment may be carried out with the imparipinnate leaf of *Sambucus nigra.* Its petiole is split longitudinally for a few centimetres and half removed. The remaining half is set in a solution of eosin. The solution is rapidly drawn up the wood-capillaries of the intact half-petiole, and soon appears in the veins of the pinnæ on the same side of the leaf, beginning with the lowest, and gradually working up into the upper ones. Finally it appears in the terminal pinna. All this while the veins of the pinnæ on the other side remain uncoloured. Now, however, the eosin begins to debouch into the base of the uppermost of these pinnæ and spreads through its veins; finally it makes its way down the offside of the rachis to the bases of the lower pinnæ, and from thence spreads into their veins. In this case we see very clearly how transpiration actuates an upward current on one side and a downward current on the other. It is interesting to note that if the terminal pinna and its stalk is removed the eosin does not appear in the pinnæ of the second side, or only after a considerable time when the small anastomosing conducting tracts are utilised.

Luise Birch-Hirschfeld recently also describes many experiments with herbaceous and woody plants, tracing the path of the reversed current by means of lithium nitrate and eosin.

In all these cases the tension of the sap determines the flow from a source wherever situated, and transpiration from the leaves, or parts of leaves, which are not supplied with liquid water from without draws the water through the plant along the channels of least resistance. Hence it is that if the cut vein of a lateral pinna provides the point of entry, the solution may pass backwards in some of the conducting tracheæ, leaving others quite uncoloured, so that only some of the veins of the pinna are injected. The injected tracts bring the solution down the rachis and petiole into the stem, while a few or many, as the case

may be, remain filled with colourless liquid, presumably the sap drawn upward to supply the transpiring surfaces of the leaf. Generally the coloured liquid descends an appreciable distance in the tracheæ of the stem before it begins to rise in the ascending current, mounting to other transpiring leaves. As a rule after some time—depending on the rate of transpiration and the amount of water supplied by the roots—the presence of the coloured liquid may be demonstrated in certain continuous series, or filaments of tracheæ in several bundles of the lower parts of the stems. Similarly, if tubers or rhizomes are present examination of these parts, after a suitable interval, will show that many of their filaments of tracheæ are injected. Meanwhile the parts above the supplying leaf become coloured, and it will be seen that the distribution of coloured tracheæ is decided by the anatomical connections of those filaments of tracheæ which directly convey the coloured liquid from the point of supply through the petiole to the stem. In tracing the path of the solution one is impressed with the fact that the path of least resistance is by no means always the shortest path in the wood. Transverse motion across several tracheæ seldom occurs, and the separate linear series of conducting tracheæ are practically isolated from each other laterally. Here we may recall Strasburger's experiments showing the very great resistance offered to the flow of water in a transverse direction in the wood of trees. This isolation of the separate filaments of tracheæ in the leaf and in the stem enables the tension developed by the transpiring cells of the leaves, while it raises a column of water in one series of tracheæ, to draw down a solution in a neighbouring filament of tracheæ terminating above in some local supply. If the anatomical connection of the two series is located in a subterranean organ the tracheæ of the subterranean organ may become filled from that supply.

So far the evidence of reversed flow in the water-conducting tracts which we have been considering has been derived from plants under artificial conditions—plants whose conducting tracts have been cut into and otherwise interfered with. Is there any evidence that reversal of the transpiration-current normally occurs in uninjured plants?

Some recent work on the transmission of stimuli seems to me to indicate that these reversals are continually occurring in normally growing plants.

The first piece of work to which I would direct your attention is that of Ricca on Mimosa. It has long been known that the stimulus which causes the folding of the pinnules and the bending of the petioles of Mimosa could traverse portions of the petioles or stems which had been raised to such a temperature as would kill the living elements in these organs. Notwithstanding that observation, Haberlandt's view, that the stimulus is transmitted as a wave of pressure through certain tubular elements of the bast, was generally accepted as the least objectionable of any of the theories which had been put forward to explain this transmission. Ricca saw that, among other difficulties, the slowness of transmission—never more than 15mm. per second—was a grave objection to this view. Accordingly working with a woody species of Mimosa—*Mimosa Spegazzinii*—he removed the whole bast and outer

tissues of the stem for many centimetres—viz., twenty-three—and was able to show that the stimulus was still transmitted. Similarly he found that the stimulus was transmitted through narrow strips of the wood from which even the pith had been removed. These experiments and others in which the transmitting organ had been killed for a considerable length caused Ricca to recognise that the stimulus is transmitted in the wood and not in the bast, as had been previously held. Thus he was led to assign the transmission to the transpiration-current. He was able to confirm this conjecture by showing that the transmission to the various leaves of a plant is largely controlled by the rate of the transpiration from the individual leaves. Thus, other things being equal, a rapidly transpiring leaf receives the stimulus sooner than a sluggishly transpiring one equidistant from the point of stimulation. He further was able to show that the stimulus may be transmitted through a glass tube filled with water, just as it is transmitted through a dead portion of the stem. Evidently a hormone set free into the transpiration-stream is the long-sought-for mechanism by which the stimulus is transmitted throughout Mimosa.

As the stimulus travels both in a basipetal and acropetal direction we may assume that movement of the transpiration-stream in a downward direction is of normal occurrence in plants.

Contemporaneously with, and subsequently to, Ricca's important work on Mimosa, experimental evidence has been accumulating to indicate that the transmission of other stimuli—viz., phototropic, traumatotropic, thigmotropic, and geotropic—is effected by means of the passage of a dissolved substance. Boysen-Jensen appears to have been the first to announce that phototropic and geotropic stimuli may be transmitted across protoplasmic discontinuities. Paál emphasised this by showing that these stimuli are able to pass a disc of the tissue of *Arundo donax* impregnated with gelatine, which is interposed between the receptive and responding regions. These observations rendered the view that the stimulus is transmitted in the form of a hormone extremely probable; and later Stark showed that this hormone is thermostable, just as Ricca had done in the case of the hormone of Mimosa. Another very interesting point discovered by Stark—working with traumatic stimuli—is that the hormones are to a certain extent specific. Thus if the perceptive tip of a seedling is removed from one plant and affixed in position on another, the certainty of the response depends on the genetic affinity of the two plants.

In all these cases it seems certain that the perceptive tissues are the point of origin, when stimulated, of a dissolved substance, the hormone, which makes its way to the motile tissues and releases the response.

In the case of Mimosa just alluded to, and of the labellum of Masdevallia examined by Oliver, there is direct evidence that the transmission of the hormone is effected by the vascular bundles. In Mimosa the channels are more precisely localised as being the tracheæ of the wood. Furthermore, the rapidity of transmission renders it certain that simple diffusion through the tissues of the plant will not account for the process. Some recorded velocities of transmission are here enumerated for the sake of comparison:—

Plant	Nature of Stimulus	Transmission Time in secs. per mm.
Mimosa	Heat	0.07
Drosera	Chemical	6.00
Seedling	Light	180–300
,,	Gravity	300
Tendril	Contact	17
Diffusion in tissue		2250–3600

There is thus every reason to believe that the transmission of stimuli generally through the tissues of the higher plants is effected by the conveyance of a hormone in the wood of the vascular bundles from the receptive to the motile regions, and whenever this transmission is in a downward direction evidence is afforded of the downward movement of water in the tracheæ. It is reasonable to suppose that this downward current is able to carry organic foodstuffs as well as hormones.

Thus the evidence for the existence of a backward flow of water in the tracheæ of wood, in addition to the more obvious upward stream, is convincing. With regard, however, to the mechanism by which the backward stream is supplied we have but scant information.

The volume-changes of leaves which Thoday has recorded are suggestive in this connection. These changes he found of various magnitudes, occurring simultaneously in different or in the same leaves. They may cause a linear contraction amounting to 2.5 per cent. in ten minutes, and may produce a volume contraction of 7 per cent. in the same time. The water corresponding to this volume-change in the cells of the leaf if transmitted into the tracheæ would produce a considerable downward displacement, as may be seen from the following figures:—

Name of Plant	Volume of 1 per cent. contraction in mm^3.	Cross-section of tracheæ in petiole in mm^2.	Downward movement in cm.
Aucuba japonica	22.8	0.05	45.6
Solanum tuberosum ...	28.0	0.07	40.0
Syringa vulgaris	42.15	0.013	16.5
Acer macrophyllum ...	42.2	0.22	19.2

If these changes in volume are caused by, or accompanied with, a development of permeability of the contracting cells, evidently a backward movement of organic substance having a velocity of about 120cm. and more per hour would be produced.

It is possible that the tension which causes these contractions of the leaf-cells at the same time acts as a stimulus to increase the permeability of the plasmatic membranes of the cells; and so one might imagine that the development of a certain tension would automatically release organic substances from the cells and draw them through the tracheæ downwards. Direct experiment on this point presents difficulties, but it may be worth recording that when the internal osmotic pressure of

the leaf-cells was overbalanced by an external gas-pressure, the water pressed from the cells and forced out of the tracheæ of the supporting stem was found to be practically pure, and if it contained carbohydrates they were in such small quantities that no reduction could be detected with Benedict's solution either before or after inversion. This experiment was repeated several times with branches of *Sambucus nigra* and *Tilia americana*. The cut branch, well supplied with water, was first exposed for several hours to conditions favourable to photosynthesis, and then either immediately or after a sojourn in darkness subjected to the gas-pressure. A pressure of thirteen atmospheres was found sufficient to drive water back from the leaves out of the stem.

Of course the conditions of this experiment are not those obtaining in the normal plant, where during transpiration the volume of a leaf, or part of a leaf, changes. In the transpiring plant we can also imagine the accumulation of a substance or an ion which would give rise to an alteration of the permeability of the plasmatic membranes of the leaves.

When, in order to imitate these conditions, the cells of the leaves in the foregoing experiment are rendered permeable by the introduction of a little toluene into the pressure-chamber, the application of a smaller pressure is sufficient to press the cell-contents into the water-channels, and liquid emerges from the base of the stem which readily reduces Benedict's solution.

In the same way, if a pinna of *Sambucus nigra* is surrounded with toluene vapour, transpiration from the adjacent pinnæ draws back the cell-contents of the toluened pinna, and afterwards their track in the wood of the vascular bundles of the rachis may be traced by the browning of this tissue.

Another possibility presented itself—viz., that the direction of the current might act as a stimulus regulating the permeability of the cells in contact with the tracheæ. To test this, short lengths of stem set in their normal position were supplied, first through their lower and afterwards through their upper end, with distilled water. In neither case could carbohydrates be detected in the issuing stream.

The foregoing short consideration of some recent physiological work leads us, then, to the following conclusions:—

The transport of the organic substances needed in the distal growing regions is effected through the tracheæ of the wood. The substances travel dissolved in the water filling these channels, which is moved by transpiration, expansion of the growing cells, or root pressure.

Physical considerations forbid us admitting that sufficiently rapid transport can be afforded by the bast either for the observed upward or downward distribution of organic substance.

The existence of downward as well as upward movement of water in the tracheæ of the wood may be demonstrated by suitable experimental means, and may be inferred by the transport of hormones in the wood.

The occurrence of local contractions in leaves suggests that local increases of permeability supply dissolved organic substances to the distal ends of certain of the filaments of tracheæ. The tension de-

veloped by the transpiration of other regions draws these along downward as well as upward channels in the wood.

In thus ruling out the participation of the bast in the longitudinal transport of organic substances in plants one naturally is forced to speculate on its probable function. Its distribution and conformation are such that, while it possesses a very small cross-section, it appears with the other living elements of the vascular bundles, medullary rays, wood-parenchyma, &c., to present a maximum surface to the tracheæ.

This large surface may find explanation in the necessity of interchange between the living cells and dead conduits. The colloidal contents of the former render this process slow, hence the necessity for the large surface of interchange to enable sufficient quantities of organic substances to be abstracted from and introduced into the tracheæ to meet the needs of the plant.

Before concluding I would like to add that the experimental work carried out on this matter would have been quite impossible for me were it not for the assistance and ingenuity of Mr. N. G. Ball. He has also contributed materially by his criticisms and suggestions.

EDUCATIONAL AND SCHOOL SCIENCE.

ADDRESS TO SECTION L (EDUCATIONAL SCIENCE) BY

SIR RICHARD GREGORY, F.R.A.S., F.INST.P.,

PRESIDENT OF THE SECTION.

THE Educational Science Section of the British Association attains its majority this year; and as a member privileged to assist in its birth, and associated with it in one capacity or another throughout its life, it is difficult to resist the temptation to survey its growth and manifold activities with the object of making performance during the years of adolescence the ground of promise for the future. But though this may be an appropriate theme to expound when an organisation has passed naturally through the various stages from infancy to maturity, it is not so apt on the present occasion; for, trite as is the simile, it is true to say that, like Pallas Athene from the head of Zeus, this section sprang into being fully grown and clothed, and its form to-day is much the same as it was twenty-one years ago.

Those of us who have been constant votaries at the new shrine then erected can recall many offerings which the goddess of wisdom would approve—seeds and flowers and fruits from the extensive and diversely fertile fields in which educational work is carried on. We have also seen a succession of distinguished presidents, too many of whom, however, have taken only a transient part in the activities of the section, coming before us for a single session and then passing from our view. The other sections of the Association are schools in which scientific workers graduate and from which they are never entirely separated, whether they reach the dignity of the presidential chair or not. For some reasons it is, perhaps, to be regretted that our section has not hitherto been so self-sufficing in the supply of presidents, but fertilisation from other fields has its advantages, and our visitors have always brought us stimulating principles of growth, so that I follow them with much diffidence, particularly as this is the first time on which one who has been Secretary and Recorder of the section has attained to the honour of the presidency.

The section was established to consolidate the claims staked out by workers in different educational provinces, and promote common interest in their development as a whole. As Professor H. E. Armstrong explained at the opening meeting, it was proposed to devote attention to education in all its branches with the object of introducing scientific conceptions into every sphere of educational activity; that is, concep-

tions which imply such exact and profitable treatment of a subject as should come from full knowledge. Educational science signifies, however, much more than methods of teaching or the theory of the curriculum. It involves conditions of physical, mental, and moral health, with their manifold types and variations, and the determination of the most appropriate, and therefore most effective, factors of growth at every stage of development. In its present stage educational science must be largely empirical, but in this respect it does not differ from meteorology, for example; and the laws which govern the perpetually varying contents and conditions of a child's mind are not much less precisely known or applied than those by which atmospheric changes are determined. The essential attribute of all scientific investigation is the spirit of discovery, and the standards by which the results are judged are not necessarily those of practical application. Teachers too often forget this when contributions to knowledge of mental processes are brought before them. Like engineers and medical practitioners, they expect science to provide things which are directly useful, whereas their duty is to discover possibilities in results achieved. Their own work is practical or clinical, and as such may help to test structures or prescriptions, but teaching is an art rather than a science, though like all arts its advances are most secure when they are founded upon scientific principles.

It is not necessary, however, to discuss whether research in education belongs to pure or to applied science, whether, to use the distinction now adopted in other departments of progressive knowledge, it is scientific or industrial. It is scientific in so far as it follows scientific methods, reveals new facts, arrives at clear conclusions, and suggests consequences which are afterwards confirmed in application. Fortunately, it is possible to do these things without possessing complete knowledge. Hipparchus was able to determine the periods of revolutions of the planets with remarkable accuracy, and his values differ very slightly from those accepted at the present day, but it was not until eighteen hundred years later that Newton discovered the law of gravitation by which the movements of these and other bodies in the solar system are governed. So Plato and Aristotle had conceptions of education and the theory of conduct which are as true to-day as when they were expounded, because, though the conditions are different, the fundamental human qualities which it is desired to stimulate and make permanent for life are the same.

While, however, we very willingly pay tribute to the wisdom of the intellectual giants of the past, we should not let it control present thought or future policy. The scheme of education outlined by Plato in his 'Republic' was designed for the training of gentlemen of means and leisure, and it left out of account altogether everything of the nature of manual occupation or professional equipment. It was a class education adapted to the needs and circumstances of the time, and its interest to us is chiefly academic or historic. Plato and other Greek thinkers were mostly concerned with education as a moral or civic process throughout life, and the school was only one stage affecting continuous development. It was believed that to apprehend scientific principles and laws, or appreciate philosophic reasoning, required mature

minds; therefore the serious study of these subjects was reserved for manhood and had no place in the school.

Modern science differs greatly from what was known to the Greeks, particularly in the use of experimental methods of inquiry; and if Plato were now constructing an educational system adapted to existing needs he would no doubt readjust its position in the curriculum. Yet there is sound psychology in the postponement of the consideration of laws and systems to late stages of a school course. Knowledge begins with sense perception, and intelligent appreciation of laws expressing general relationships or affinities, or the recognition of the place of such laws in a system, can be expected only from gifted pupils. It is the business of education to promote the right adjustment between the developing human organism and its surroundings, and this implies that the nourishment provided at all stages of growth should be not only such as supplies the needs of the moment, but also builds up strength to live a full life under the conditions of the times. Whether we consider the practical education or training by which uncivilised man learns to supply his needs, the humanistic conceptions of ancient Greece, medieval education, or modern systems, the aim is the same, namely, to create worthy members of particular social fabrics—to adapt people to meet the necessities of life and respond to the best influences of existing circumstances. It is true that Kant thought children should be educated not for the present but for a possibly improved condition of man in the future, yet he himself advocated the cultivation of natural ability to meet practical needs of life.

Education may, therefore, be defined as the deliberate adjustment of a growing human being to its environment; and the scope and character of the subjects of instruction should be determined by this biological principle. What is best for one race or epoch need not be most appropriate for another, but always the aim should be to give the pupil as many points of contact with the world around him as may be profitably developed during his school career. This does not mean, of course, that his vision is to be confined to contemporary necessities or his thoughts to provincial or even national fields. The resources available for his instruction and guidance comprise the wisdom and experience of the past as well as the power of the present, and in their extensive and varied character they now provide teachers with educational opportunities richer and fuller than those of any other period of the world's history. Literature and art form noble domains of the heritage into which the child of to-day is born, but they were mostly planted long ago, and their shapes have not been altered much in modern times. Science has, however, transformed the whole landscape entrusted to it, and the realm of its productivity is continually extending. It is a kingdom potent with possibilities for good or evil—an inheritance which cannot be renounced—and to let any of our children grow up unfamiliar with their entailed possession is to neglect an obvious duty.

The essential mission of school science is thus to prepare pupils for civilised citizenship by revealing to them something of the beauty and the power of the world in which they live, as well as introducing them to the methods by which the boundaries of natural knowledge have been

extended and Nature herself is being made subservient to her insurgent son. We live in a different world to-day from that of medieval times, when the *trivium* of grammar, logic, and rhetoric, with the *quadrivium* of arithmetic, geometry, music and astronomy, comprised the subjects of a complete education in the sciences as well as in letters—different indeed from what it was only a century ago. The influence of science is now all-pervading, and is manifest in all aspects of human activity, intellectual and material. Acquaintance with scientific ideas and methods and applications is forced upon everyone by existing circumstances of civilised life with its facilities for rapid transport by air, land, or sea, ready communication by telephone or telegraph, and other means by which space and time have been brought under control and man has assumed the mastership of his physical and social destiny. Science permeates the atmosphere in which we live, and those who cannot breathe it are not in biological adjustment with their environment—are not adapted to survive in the modern struggle for existence.

School instruction in science is not, therefore, intended to prepare for vocations, but to equip pupils for life as it is and as it soon may be. It is as essential for intelligent general reading as it is for everyday practical needs; no education can be complete or liberal without some knowledge of its aims, methods, and results, and no pupil in primary or secondary schools should be deprived of the stimulating lessons it affords. In such schools, however, the science to be taught should be science for all, and not for embryonic engineers, chemists, or even biologists; it should be science as part of a general education—unspecialised, therefore, and without reference to prospective occupation or profession, or direct connection with possible university courses to follow. Less than 3 per cent. of the pupils from our State-aided secondary schools proceed to universities, yet most of the science courses in these schools are based upon syllabuses of the type of university entrance examinations—syllabuses of sections of physics or chemistry, botany, zoology, and so forth—suitable enough as preliminary studies of a professional type to be extended later, but in no sense representing in scope or substance what should be placed before young and receptive minds as the scientific portion of their general education. Such teaching excuses the attitude of many modern Gallios among schoolboys caring ‘ for none of those things.’ The needs of the many are sacrificed to the interests of the few, with the result that much of the instruction is inept and futile whether judged by standards of enlightenment or of stimulus. Exceptional pupils may profit by it, but to others, and particularly to teachers of literary subjects in the school curriculum, it often appears trivial or sordidly practical, and is usually spiritless—a means by which man may gain the whole world, but will lose his soul in the process.

This impression is not altogether unjust, and the teaching of recent years has tended to accentuate it. The extent of school science is determined by what can be covered by personal observation and experiment—a principle sound enough in itself for training in scientific method, but altogether unsuitable to define the boundaries of science in general education. Yet it is so used. Every science examination qualifying for the

First School Certificate, which now represents subjects normally studied up to about sixteen years of age, is mainly a test of practical acquaintance with facts and principles encountered in particular limited fields, but not a single one affords recognition of a broad and ample course of instruction in science such as I believe is required in addition to laboratory work. I have not the slightest intention or desire to suggest that practical work can be dispensed with in the teaching of any scientific subject, but I do urge that it becomes a fetish when it controls the range of view of the realm of natural knowledge capable of being opened for the best educational ends during school life.

Advocates of both literary and scientific studies now agree that science should be integrally and adequately represented in the educational course of all pupils up to the age of sixteen, and the Headmasters' Conference has subscribed to this view, as well as suggested the scope of the course, in the following resolutions:—

(1) That it is essential to a boy's general education that he should have some knowledge of the natural laws underlying the phenomena of daily life, and some training in their experimental investigation.

(2) That, in the opinion of this Conference, this can best be ensured by giving to all boys adequate courses of generalised science work, which would normally be completed for the ordinary boy at the age of sixteen.

(3) That, after this stage, boys who require it should take up science work of a more specialised type, while the others should for some time continue to do some science work of a more general character.

As indicated in these resolutions, it is now generally recognised by educationists that up to the age of about sixteen years there should be no specialisation in school studies. The First School Examination was organised with this end in view, and seven examining bodies have been approved by the Board of Education to test the results of instruction given in (1) English subjects, (2) languages, (3) mathematics and science, which constitute the three main groups in which candidates are expected to show a reasonable amount of attainment. The number of candidates who presented themselves at examinations of the standard of First School Certificates last year was about 42,000; and of this number, 12,500 took papers in sections of physics, 13,000 in chemistry, 11,400 in botany, 5,000 physics and chemistry combined under experimental science, 113 natural history of animals, 31 geology, and 3 zoology.

These numbers may be taken as a fair representation of the science subjects studied in most of our secondary schools, and they suggest that general scientific teaching is almost non-existent. Botany is a common subject in girls' schools, but the instruction in science for boys is limited to parts of physics and chemistry. The former subject is usually divided into mechanics and hydrostatics; heat; sound and light; and electricity and magnetism; and candidates are expected to reach a reasonable standard in two of these sections. They may, therefore, and often do, leave school when their only introduction to science is that represented by the study of mechanics and heat, and without the slightest knowledge of even such a common instrument as an electric

bell, while the ever-changing earth around them, and the place of man in it, remain as pages of an unopened book. They ask for bread, and are given a stone. General science covering a wide field is practically unknown as a school subject, and even general physics rarely finds a place in the curriculum because questions set in examinations are, to quote from the Cambridge Locals Regulations, 'principally such as will test the candidate's knowledge of the subject as gained from a course of experimental instruction.' This condition reduces the range of instruction in such a subject as physics to what can be covered in the laboratory, and makes a general course impossible; for time and equipment will not permit every pupil to learn everything through practical experiment. Reading or teaching for interest, or to learn how physical science is daily extending the power of man, receives little attention because no credit for knowledge thus gained is given in examinations.

One or two examining bodies have introduced general science syllabuses covering the rudiments of physics and chemistry as well as of plant and animal life, but even in these cases most of the subjects must be studied experimentally, and no place is found for any other means of acquiring knowledge. The result is that few schools find it worth while from the point of view of examination successes to attempt to cover such schemes of work. Moreover, no clear principle can be discerned by which the syllabuses are constructed. General science should be more than an amorphous collection of topics from physics and chemistry, with a little natural history thrown in as a sop to biologists. It should provide for good reading as well as for educational observation and experiment; should be humanistic as well as scientific. The subject which above all others has this double aspect is geography; so truly, indeed, is this the case that in the First School Examinations it may be offered in either the English or the Science group. Practically all the subjects of a broad course of general science are of geographical significance, inasmuch as they are concerned with the earth as man's dwelling-place, and the scene of his activities. Rightly conceived, geography can be made the earliest means of education as both Comenius and Locke regarded it, and it can be used as the unifying principle of all the generalised scientific instruction in schools. It is now much more than travel stories of the type of Sir John Mandeville's medieval miscellany, or mere lists of capes and rivers, countries and cities. It provides interesting subjects for laboratory exercises and field work, and the results of observation and experiment are seen to be of use in understanding what is going on in the earth as the result of both natural and human agencies. A school course which would cover all the science required for the study of geography conceived as a branch of knowledge concerned with the natural environment of man and the inter-relations between him and those circumstances would not only be educational in the broadest sense, but would also be the best groundwork for effective teaching of geography, history, and other humanistic studies. It would make science a natural part of a vertebrate educational course instead of specialised and exclusive as it tends to be at present.

There is very present need for the reminder that science is not all

measurement, nor is all measurement science. Observation also is not merely looking at things, but examining them with a seeing mind and clear purpose. School science to-day, however, is almost entirely concerned with measurement, and pupils will cheerfully record that they observed what they could never possibly have seen (as, for example, the production of an invisible gas), while they continually carry out experiments which to them have no other purpose than that of occupying their time, or to provide them with details demanded by examination questions. In the great majority of secondary schools science signifies chiefly quantitative work in physics and chemistry—laboratory exercises and lessons bearing upon them—and rarely is any attempt made to show the pupils what a wonderful world we live in, or what science has done, and is doing, for them in their everyday life. Much of the work described as physics really belongs to mensuration, and has no claim upon the time devoted to science, though it helps to fix instruction in arithmetic or other branches of school mathematics. There is, indeed, no virtue in measuring and weighing in the absence of intelligent appreciation of the objects for which such operations are performed, or of interest in them.

In the usual course of physics, from fundamental measurements and mechanics to heat, possibly with light and sound, and magnetism and electricity, to follow, though relatively few pupils get beyond the heat stage, natural or psychological needs are sacrificed to logical sequence. It cannot be reasonably suggested that the order in which these subjects are prescribed has any relation to mental growth, or that the topics selected from them are such as appeal to early interests. Few pupils of their own volition wish to determine specific gravities, investigate the laws of motion, calculate specific and latent heats, and so on, at the stage of instruction in science at which these matters are usually studied, and from the point of view of educational value most of them would be more profitably employed in becoming acquainted with as wide a range as possible of common phenomena and everyday things—all considered as qualities to stimulate attention instead of quantities to be measured with an accuracy for which the need cannot be seen and by methods which easily become wearisome. The 'Investigators' appointed by the Board of Education in 1918 to report upon the papers set in examinations for the First School Certificate were right when they expressed their opinion 'that the early teaching of physics has suffered from too great insistence on more or less exact quantitative work, to the neglect of qualitative or very roughly quantitative experiments illustrating fundamental notions.' By the prevailing obsession in regard to quantitative work the pupil is made the slave of the machine, and appliances become encumbrances to the development of the human spirit.

The prime claim of science to a place in the school curriculum is based upon the intellectual value of the subject matter and its application to life. This conception of education through science as the best preparation for complete living was Herbert Spencer's contribution to educational theory; and to its influence the introduction of science into the school is largely due. Spencer's doctrine was in accord with the

principles of Pestalozzi as to the sequence in which facts and ideas should be presented and be related to stages of development, in order to be effective in creating or fostering natural interests in the mind of the child. Scientific instruction implies, therefore, not alone knowledge that is best for use in life, but knowledge adapted to the normal course of mental development. Both substance and method should be judged by the criterion of what is of greatest immediate worth or nearest to the pupil's interests at the moment. When this standard of psychological suitability is applied to the school science courses now usually followed, it must be confessed that they rarely reach it, many topics and much material being remote from the pupil's natural interests and needs.

The truth is that in the design of science courses for schools 'trial-and-error' methods have been followed. In the absence of accurate knowledge these are the only possible methods of construction, but sufficient is now known of child psychology to produce a scheme of scientific instruction which represents not merely the views of advocates of particular subjects, but is biologically sound because it is in accord with the principles of mental growth, and, therefore, with those of educational science. When instruction in science was first introduced into schools its character was determined by insight and conviction rather than by mental needs or interests; so later, when practical work came to be regarded as an essential part of such instruction, its nature and scope represented what certain authorities believed pupils should do, instead of what they were capable of doing with intelligence and purpose. Practical chemistry became drill in the test-tubing operations of qualitative analysis, and the result was so unsatisfactory from the points of view of both science and education that when Professor Armstrong put forward a scheme of instruction devised by him, in which intelligent experimentation took the place of routine exercises, acknowledgment of its superior educational value could not be withheld, and for thirty years its principles have influenced the greater part of the science teaching in our schools.

In its aims the 'heuristic' methods of studying science energetically advocated by Professor Armstrong were much the same as those associated with the names of other educational reformers. Education in every age tends to a condition of scholasticism, and practical science teaching is no exception to this general rule, its trend being towards ritual, after which a revolt follows in the natural order of events. Comenius, with his insistence upon sense perception as the foundation of early training—'Leave nothing,' he said, 'until it has been impressed by means of the ear, the eye, the tongue, the hand'; John Dury among the Commonwealth writers who urged that pupils should be guided to observe all things and reflect upon them; Locke, with his use of sciences not to bring about 'a variety and stock of knowledge, but a variety and freedom of thinking'; and Rousseau, who would 'measure, reason, weigh, compare,' not in order to teach particular sciences, but to develop methods of learning them—all these were in different degrees apostles of the same gospel of education according to Nature, and the development of a scientific habit of mind as the intention of instruction. What Rousseau persistently urged in this direction

was clearly formulated by Spencer in the words, 'Children should be led to make their own investigations, and to draw their own inferences. They should be *told* as little as possible, and induced to *discover* as much as possible'—principles which cover all that is implied in what has since been termed 'heuristic' teaching.

Professor Armstrong's particular contribution to educational science consisted in the production of detailed schemes of work in which these principles were put into practice. Ideas are relatively cheap, and it needs a master mind to make a coherent story or useful structure from them. This was done in the courses in chemistry outlined in Reports presented to the British Association in 1889 and 1890, and the effect was a complete change in the methods of teaching that subject. 'The great mistake,' said Professor Armstrong, 'that has been made hitherto is that of attempting to teach the elements of this or that special branch of science; what we should seek to do is to impart the elements of scientific method and inculcate wisdom, so choosing the material studied as to develop an intelligent appreciation of what is going on in the world.' One feature of heuristic instruction emphasised by its modern advocate, but often neglected, is that which it presents to the teaching of English. Accounts of experiments had to be written out in literary form describing the purpose of the inquiry and the bearing of the results upon the questions raised, and wide reading of original works was encouraged. A few years ago English composition was regarded as a thing apart from written work in science, but this should not be so, and most teachers would now agree with the view expressed by Sir J. J. Thomson's Committee on the Position of Natural Science in the Educational System of Great Britain that 'All through the science course the greatest care should be taken to insist on the accurate use of the English language, and the longer the time given to science the greater becomes the responsibility of the teacher in this matter. . . . The conventional jargon of laboratories, which is far too common in much that is written on pure and applied science, is quite out of place in schools.'

When heuristic methods are followed in the spirit in which they were conceived, namely, that of arousing interest in common occurrences, and leading pupils to follow clues as to their cause, as a detective unravels a mystery, there is no doubt as to their success. No one supposes that pupils must find out everything for themselves by practical inquiry, but they can be trained to bring intelligent thought upon simple facts and phenomena, and to devise experiments to test their own explanations of what they themselves have observed. It is impossible, however, to be true to heuristic methods in the teaching of science and at the same time pay addresses to a syllabus. A single question raised by a pupil may take a term or a year to arrive at a reasonable answer, and the time may be well spent in forming habits of independent thinking about evidence obtained at first-hand, but the work cannot also embrace a prescribed range of scientific topics. Yet under existing conditions, in which examinations are used to test attainments, this double duty has to be attempted by even the most enlightened and progressive teachers of school science. There can, indeed, be no profitable training in research methods in school laboratories under the

shadow of examination syllabuses. Where there is freedom from such restraint, and individual pupils can be permitted to proceed at their own speeds in inquiries initiated on their own motives, success is assured, but in few schools are such conditions practicable; so that, in the main, strict adherence to the heuristic method is a policy of perfection which may be aimed at but is rarely reached.

A necessary condition of the research method of teaching science is that the pupils themselves must consider the problems presented to them as worth solving, and not merely laboratory exercises. Moreover, the inquiries undertaken must be such as can lead to clear conclusions when the experimental work is accurately performed. It may be doubted whether the rusting of iron or the study of germination of beans and the growth of seedlings fulfils the first of these conditions, and the common adoption of these subjects of inquiry is due to custom and convenience rather than to recognition of what most pupils consider to be worth their efforts. It needed a Priestley and a Lavoisier to proceed from the rusting of iron to the composition of air and water, and even such an acute investigator as Galileo, though well aware that air has weight, did not understand how this fact explained the working of the common suction pump. If research methods are to be followed faithfully, and what pupils want to discover about natural facts and phenomena is to determine what they do, then teachers must be prepared to guide them in scores of inquiries both in and out of the laboratory. Under the exigencies of school work it is impracticable to contemplate such procedure, and all that can be usefully attempted is to lead pupils to read the book of Nature and to understand how difficult it is to obtain a precise answer to what may seem the simplest question.

The mission of school science should not, indeed, be only to provide training in scientific method—valuable as this is to everyone. Such training does cultivate painstaking and observant habits, and encourages independent and intelligent reasoning, but it cannot be held in these days that any one subject may be used for the general nourishment of faculties which are thereby rendered more capable of assimilating other subjects. Modern psychology, as well as everyday experience, has disposed of this belief. If the doctrine of transfer of power were psychologically sound, then as good a case could be made out for the classical languages as for science, because they also may be taught so as to develop the power of solving problems and of acquiring knowledge at the same time. When, therefore, advocates of particular courses of instruction state that they do not pretend to teach science, but are concerned solely with method, they show unwise indifference to what is known about educational values. Locke's disciplinary theory—that the process of learning trains faculties for use in any fields, and that the nature of the subject is of little consequence—can no longer be entertained. It has now to be acknowledged that information obtained in the years of school life is as important as the process of obtaining it; that, in other words, subject matter as well as the doctrine of formal discipline must be taken into consideration in designing courses of scientific instruction which will conform to the best educational principles.

So long ago as 1867 the distinction between subject and method was clearly stated by a Committee of the British Association, which included among its members Professor Huxley, Professor Tyndall, and Canon Wilson. It was pointed out that general literary acquaintance with scientific things in actual life, and knowledge relating to common facts and phenomena of Nature, were as desirable as the habits of mind aimed at in scientific training through 'experimental physics, elementary chemistry, and botany.' The subjects which the Committee recommended for scientific information, as distinguished from training, comprehended 'a general description of the solar system; of the form and physical geography of the earth, and such natural phenomena as tides, currents, winds, and the causes that influence climate; of the broad facts of geology; of elementary natural history with especial reference to the useful plants and animals; and of the rudiments of physiology.' If we add to this outline a few suitable topics illustrating applications of science to everyday life, we have a course of instruction much more suitable for all pupils as a part of their general education than what is now commonly followed in secondary schools. It will be a course which will excite wonder and stimulate the imagination, will promote active interest in the beauty and order of Nature, and the extension of the Kingdom of Man, and provide guidance in the laws of healthy life.

The purpose of this kind of instruction is, of course, altogether different from that of practical experiment in the laboratory. One of the functions is to provide pupils with a knowledge of the nature of everyday phenomena and applications of science, and of the meaning of scientific words in common use. Instead of aiming at creating appreciation of scientific method by an intensive study of a narrow field, a wide range of subjects should be presented in order to give extensive views which cannot possibly be obtained through experimental work alone. The object is indeed almost as much literary as scientific, and the early lessons necessary for its attainment ought to be within the capacity of every qualified teacher of English. Without acquaintance with the common vocabulary of natural science a large and increasing body of current literature is unintelligible, and there are classical scientific works which are just as worthy of study in both style and substance as many of the English texts prescribed for use in schools. We all now accept the view that science students should be taught to express themselves in good English, but little is heard of the equal necessity for students of the English language to possess even an elementary knowledge of the ideas and terminology of everyday science, which are vital elements in the modern world, and which it is the business of literature to present and interpret.

So much has been, and can be, said in favour of broad courses of general informative science in addition to laboratory instruction and lessons which follow closely upon it, that the rarity of such courses in our secondary schools is a little surprising at first sight. Their absence seems to be due to several reasons. In the first place, the teachers themselves are specialists in physics, chemistry, biology, or some other department of science, and they occupy their own territory in school

as definitely as Mr. Eliot Howard has shown to be the behaviour-routine of birds in woods and fields. You may, therefore, have a teacher of physics who has taken an honours degree and yet knows less of plant or animal life than a child in an elementary school where Nature Study is wisely taught; and, on the other hand, there are teachers of natural history altogether unacquainted with the influence of physical and chemical conditions upon the observations they describe or the conclusions they reach. Natural science as a single subject no longer exists either in school or university, and with its division and sub-division has come a corresponding limitation of interest. No man can now be considered as having received a liberal education if he knows nothing of the scientific thought around him, but it is equally true that no man of science is scientifically educated unless his range of intellectual vision embraces the outstanding facts and principles of all the main branches of natural knowledge. It cannot reasonably be suggested that this general knowledge of science should be acquired by all if teachers of science themselves do not possess it. During the past thirty years or so there has been far too much boundary-marking of science teaching in school on account of the specialised qualifications of the teachers. What is wanted is less attention to the conventional division of science into separate compartments designed by examining bodies, and more to the whole field of Nature and the scientific activities by which man has transformed the world; and no teacher of school science should be unwilling or unqualified to impart such instruction to his pupils.

Where such teachers do exist, however, they are compelled by the exigencies of examinations to conform to syllabuses of which the boundary lines are no more natural than those which mark political divisions of countries on a map of the world. All that can be said in favour of the delimitation of territory is that it is convenient; the examiner knows what the scope of his questions may be, and teachers the limits of the field they are expected to survey with their pupils. While, therefore, it may be believed that a general course of science is best suited to the needs of pupils up to the age of about sixteen years, examining authorities recognise no course of this character, and very few schools include it in the curriculum. Expressed in other words, the proximate or ultimate end of the instruction is not education but examination, not the revealing of wide prospects because of the stimulus and interest to be derived from them, but the study of an arbitrary group of topics prescribed because knowledge of them can be readily tested. It may be urged that this is the only practicable plan to adopt if a science course is to have a defined shape, and not, like much that passes for Nature Study, merely odds and ends about Nature, without articulation or purpose. Acceptance of this view, however, carries with it the acknowledgment that expediency rather than principle has to determine the scope and character of school science, which is equivalent to saying that science has no secure place in educational theory. I prefer to believe that a school course of general science can be constructed which is largely informative and at the same time truly educational, but it must provide what is best adapted to enlarge the

outlook and develop the capacity of the minds which receive it, and not be determined by the facilities it offers for examinational tests.

A third reason for the relative absence of general scientific education in schools is the demands which the teaching might make upon apparatus and equipment. Simple quantitative work in physics, chemistry, or botany can be done in the laboratory with little apparatus, and a single experiment may occupy a pupil for several teaching periods. To attempt to provide the means by which all pupils can observe for themselves a wide range of unrelated facts and phenomena belonging to the biological as well as to the physical sciences is obviously impracticable, and would be educationally ineffective. Experiments carried out in the laboratory should chiefly serve to train and test capacity of attacking problems and arriving at precise results just as definitely as do exercises in mathematical teaching. But knowledge by itself, whether of quantitative or qualitative character, is not sufficient, and it becomes power only when it is expressed or used. Every observation or experiment carries with it, therefore, the duty of recording it clearly and fully in words or computations, or both, and if this is faithfully done laboratory work of any kind may be made an aid to English composition as well as an incentive to independent inquiry and intelligent thought.

It is very difficult, however, to devise a laboratory course of general science which shall be both coherent and educative; shall be, in other words, both extensive in scope and intensive in method. I doubt, indeed, whether any practical course can perform this double function successfully. Probably the best working plan is to keep the descriptive lessons and the experimental problems separate, using demonstrations in the class-room as illustrations, and leaving the laboratory work to itself as a means of training in scientific method or of giving a practical acquaintance with a selected series of facts and principles. The main thing to avoid is the limitation of the science teaching to what can be done practically; for no general survey is possible under such conditions. Even if two-thirds of the time available for scientific instruction be devoted to laboratory experiment and questions provoked by it, the remaining third should be used to reveal the wonder and the power and the poetry of scientific work and thought; to be an introduction to the rainbow-tinted world of Nature as well as provide notes and a vocabulary which will make classical and contemporary scientific literature intelligible. If there must be a test of attention and understanding in connection with such descriptive lessons, because of the spirit of indifference inherent in many minds—young as well as old—let it be such as will show comprehension of the main facts and ideas presented and knowledge of the meaning of the words and terms used. In this way descriptive lessons may be used to provide material for work and active thought, and light dalliance with scientific subjects avoided.

It may be urged that no knowledge of this kind has any scientific reality unless it is derived from first-hand experience, and this is no doubt right in one sense; yet it is well to remember that science, like art, is long while school life is short, and that though practical familiarity with scientific things must be limited, much pleasure and profit can be

derived from becoming acquainted with what others have seen or thought. It is true that we learn from personal experience, but a wise man learns also from the experience of others, and one purpose of a descriptive science course should be to cultivate this capacity of understanding what others have described. As in art, or in music, or in literature, the intention of school teaching should be mainly to promote appreciation of what is best in them rather than to train artists, musicians, or men of letters, so in science the most appropriate instruction for a class as an entity must be that which expands the vision and creates a spirit of reverence for Nature and the power of man, and not that which aims solely at training scientific investigators. It should conform with Kant's view that the ultimate ideal of education is nothing less than the perfection of human nature, and not merely a goal to be obtained by the select few.

The sum and substance of this address is a plea for the expansion of scientific instruction in this humanising spirit, for widening the gateway into the land of promise where the destinies of the human race are shaped. It is the privilege of a president to be to some extent pontifical—to express opinions which in other circumstances would demand qualification—and to leave others to determine how far the doctrines pronounced can be put into practice in daily life. I do not, therefore, attempt to suggest the outlines of courses of science teaching for pupils of different ages, or for schools of different types; this has been done already in a number of books and reports, among the latter being the Report of Sir J. J. Thomson's Committee on the Position of Natural Science, the Report of the British Association Committee on Science Teaching in Secondary Schools, Mr. O. H. Latter's Report to the Board of Education on Science Teaching in Public Schools, the 'Science for All' Report and Syllabus issued by the Science Masters' Association, a Board of Education Report on 'Some Experiments in the Teaching of Science and Handwork in certain Elementary Schools in London,' and one prepared for the Board by Mr. J. Dover Wilson on 'Humanism in the Continuation School.' What has been said in this address as to the need for extending the outlook of customary scientific instruction beyond the narrow range of manual exercises, manipulative dexterity, experimental ritual, or incipient research, can be both amplified and justified from these Reports. I want science not only to be a means of stimulating real and careful thinking through doing things, but also a means of creating interest and enlarging the working vocabulary of the pupils and thus truly increasing their range of intelligence. So may scientific instruction be made a power and an inspiration by giving, in the words of the Book of Wisdom (vii. 16-20):—

'an unerring knowledge of the things that are,
To know the constitution of the world and the operation of the elements;
The beginning and end and middle of times,
The alternations of the solstices and the changes of seasons,
The circuits of years and the positions of stars;
The nature of living creatures and the raging of wild beasts,
The violences of winds and the thoughts of men,
The diversities of plants and the virtues of roots.'

When school science has this outlook it will lie closer to the human heart than it does at present, and a common bond of sympathy will be formed between all who are guiding the growth of young minds for both beauty and strength. So will the community of educational aims be established and the place of science in modern life be understood by a generation which will be entrusted with the task of making a new heaven and a new earth. If these trustees for the future learn to know science in spirit as well as in truth we may look forward with happy confidence to the social structure they will build, in which knowledge will be the bedrock of springs of action and wisdom will make man the worthy monarch of the world.

THE PROPER POSITION OF THE LANDOWNER IN RELATION TO THE AGRICULTURAL INDUSTRY.

ADDRESS TO SECTION M (AGRICULTURE) BY

THE RIGHT HON. LORD BLEDISLOE, K.B.E.,

PRESIDENT OF THE SECTION.

At a critical period in the history of British agriculture you have invited one who is not an expert scientist but an ordinary country squire, intensely proud of the traditions and deeply conscious of the potentialities of the class to which he belongs, to preside over the Agricultural Section of the British Association. If in my address I fail to carry persuasion, it will not be through lack of strong convictions or of a sense of responsibility in giving utterance to them. This meeting marks the tenth anniversary of the inauguration of this Section of the Association. It may with reason be asked whether it has so far justified itself. It can only do so if the teachings of science not merely tinge but permeate ordinary British farm practice to the commercial advantage of the whole agricultural industry. It is not sufficient for scientists to preach only to the converted. Whether in the realm of animal husbandry, or in that of arable cultivation, the pursuit of scientific method must not be confined to the favoured few possessing abnormal wealth or an exceptional combination of intellectual zeal with business aptitude, but must for its full justification result in an improved general standard of farming and a largely increased output of agricultural produce at a reasonable margin of profit, in which the whole rural community participates. Considering the wealth of discovery in almost every branch of agricultural research during the last quarter of a century, and the greatly enlarged scope of scientific investigation as applied to agricultural problems during the last few years, the absorption into ordinary British farm practice of the results of such investigation is far from being commensurate with the labour or, indeed, the expense of scientific effort.

Although there is amongst farmers a growing appreciation of the value of science to their industry, there is far too wide a gap between the most enlightened and commercially successful farm practice and that of the average farmer in this country.

How is this gulf to be bridged?

My immediate predecessor in the Presidency of this Section, Mr. C. S. Orwin, in his carefully reasoned and suggestive address last year at Edinburgh, pointed out that a study of economics and the constant

recognition of dominant economic force must go hand in hand with agricultural research and education in the various branches of science upon which agriculture is based, if the latter are to receive their full fruition, and if the business of farming is to be profitably conducted. The highest skill in the task of actual production may, in the absence of efficient business management and of the organisation in the interests of the agricultural producer of the conversion, transport, distribution and sale of his produce, fail to prevent the bankruptcy court being his ultimate destination. A good recent illustration of the anomalous and unfortunate result of lack of such organisation is the sale to millers by thousands of farmers last autumn of exceptionally high quality wheat at a relatively low value, and the subsequent purchase by the same farmers, in many cases from the same source, of residual wheat offals for the feeding of their pigs or cattle at considerably higher prices than those paid to them for the whole grain; or again, the crisis among dairy farmers last spring arising out of the attempt on the part of a powerful combination of milk distributors to compel them to enter into summer milk contracts at prices which left them no prospective margin of profit, while retailing the same commodity to the public in the towns at nearly three times the price paid to the producer, thus incidentally putting a premium upon the increased importation from abroad of milk powder and other milk substitutes.

The crying need of such organisation is admitted. But how is it to be supplied? Numerous public-spirited efforts have been made for at least thirty years by the Agricultural Organisation Society and other like bodies, with Government encouragement, to develop co-operative effort among British farmers, comparable to that which has attended the same movement in Denmark, Germany, Belgium, Holland, Italy, Hungary and (in more recent years) Ireland, but without any very marked or persistent success, largely owing to the somewhat obstinate individuality and mutual suspicion of our agricultural population, and partly and chiefly owing to the lack of the initiative and control in such enterprises of outstanding and universally acknowledged leaders of indisputable integrity and business capacity.

If efficient organisation is the chief desideratum of British rural industry, and if its availability depends upon trained leadership, where is such leadership to be found?

Let us glance at the other side of the picture. There is a strong political movement in favour of Land Nationalisation. It is part of the accepted creed of organised labour in this country; it is, significantly, the chief political tenet of the two national groups of organised agricultural workers. It implies no hostility amongst its adherents to agricultural landowners, either individually or as a class. In fact, the country squire, especially one who is, so to speak, 'ascriptus glebæ'—whose family has become deep-rooted for several generations in the soil of the locality as well as in honourable traditions of public service and philanthropic utility—is, or at least, speaking generally, was (until post-War impoverishment threatened his continuing stability) an object of respect, and often of affection, among the local working population, more so very often than the farm tenants upon his estate. Every reputable

landowner who has faced the ordeal of a political contest in a purely rural constituency is conscious of the almost pathetic confidence evoked, not so much by the professions of his political faith as by the fact of his land-ownership, and the assumption of well-informed sympathy which is deemed to be associated with it. He may be stupid or reactionary, but he inspires respect for his honesty, patriotism, and unselfish devotion to duty. Yet to the advocates of Land Nationalisation in the mass he appears as an industrial parasite, a mere rent-receiver, who 'reaps where he has not sown, who gathers where he has not strawed.' He owns, it is true—in the form of land, buildings, and other farm equipment—at least two-thirds of the capital embarked in the industry of agriculture. He may derive 2 per cent. or less from his capital so invested, and live an inconspicuous life of comparative poverty, while the sale (especially in recent years) of his estate and the investment of the proceeds of sale in Government securities might treble his income and raise him to a condition of comparative affluence. But unless he is himself a farmer (which is seldom the case) he lives a life detached from the industry carried on upon his estate, and often ineffectually seeks relief from his growing poverty by attaching himself to a Property Defence League. He becomes, in fact, a mere property defender, which in a highly democratic State carries little conviction to a preponderantly urban proletariat, and tends to stimulate the activities of revolutionary propagandists. If, on the other hand, he were to stand out in the body politic as a producer, trained for his task as such, and prepared to accept the position of managing director of the great and, if well organised and directed, potentially profitable industry conducted upon his property, his position as a landowner would be far less vulnerable and his utility to the State indisputable.

The agricultural community in Britain to-day above all else needs enlightened leadership, just as agriculture needs efficient organisation; and the landowner, if, after due training, he would but take his proper position, should be both leader and chief organiser.

During the last half-century, when the financial resources of the average landowner, even during the great depression of the 'eighties and 'nineties, sufficed to furnish a competence for himself and his family, and before the growing burden of estate duty (against which he often secured the devolution of an undiminished inheritance by the annual payment of an insurance premium) threatened the dissolution of his estate, he was wont, at least in his youth, to serve his country in the Navy, the Army, or some other financially unremunerative branch of the public service, or to participate unpaid in the conduct of local government. He employed an estate agent (often a person of no agricultural training), who stood between him and the agricultural activities of his estate, in respect of which he was himself often deplorably ignorant, unbusinesslike, and unprogressive.

The War has naturally altered his outlook. It is estimated that the present rate of estate duty as levied upon a form of property of which (if adequately maintained) the net income is relatively low and the capital value disproportionately high, will, unless hereafter materially reduced, permit of no landed estate of average size and rental,

unbuttressed by external financial resources, remaining in one family for more than two generations. Effective insurance against the burden of death duties is in such cases no longer practicable. The continued employment of an estate agent who is not also an experienced farm manager is to many a luxury, of which the estate income will no longer admit. The sale of the estate is one of two alternatives: its owner-management and industrial development constitute the other. The second alternative is possible under a system either of Landlord and Tenant or of Occupying Ownership.

The relation of landlord and tenant necessarily depends for its success upon the leadership and initiative of the owner, based upon sound knowledge. It operated as a stimulant to English agriculture during the latter part of the eighteenth and the first half of the nineteenth century, because, following the example of George III., Lord Townshend, Lord Leicester, and other enlightened territorial magnates, it had become the fashion for the owner to interest himself in farming, and he consequently knew what the land was capable of, and gave a lead to his tenants. With the growing importations of grain from abroad, the increasing prosperity of the industrial population at the expense of the countryside, and especially in consequence of the agricultural depression during the last two decades of the nineteenth century, the landowner lost faith in himself and in his true vocation, and had neither the knowledge nor the inclination to give his tenants the lead which they required. It became, in fact, easier for him, by remitting rents and acquiescing in the farmer's desire to lay his land down to grass, to obtain the reputation of a 'good' landlord, an expression which meant in all too many cases his abandonment of leadership and his surrender to ignorant or indolent prejudice. Where neither leadership nor rent remission were forthcoming his old tenants were ruined. The prime condition under which farm tenancy can prosper is the owner's knowledge and management of his estate, similar to that exercised by the manager of an industrial company in relation to his business. The owner, in fact, if he carried out to the full the possibilities of his position, ought continuously, with the knowledge and experience which would render intervention acceptable, to be guiding his tenants in the way of improving their business by the constant application of science to farm practice, the employment of labour-saving machinery, the discovery of new markets, and, above all, by the development of co-operation. If he had but the knowledge and the faith he could have done much during the last half-century by insisting upon the proper education of his tenants' sons before they in their turn became occupiers of his estate holdings, or even by looking to the agricultural colleges for the provision of fresh blood and enterprise among his tenantry, himself selecting at times a likely youth from the human output of such institutions.

Whereas at the end of the eighteenth and the beginning of the nineteenth century certain progressive English landowners were definitely and admittedly the leaders of the industry, to-day, and for the past sixty years, landowners have ceased to lead. Coke of Norfolk and his contemporaries in introducing developments which benefited

the whole industry also benefited themselves and their whole environment, because these improvements were introduced on sound business lines. Land to-day in the hands of British landowners is more than ever an amenity, and although there are many whose serious impoverishment operates as an inducement to put their estates upon a business footing, they are sadly conscious that they have not the knowledge to do so. The excessive development of urban and industrial interests, coupled with the relatively severe neglect of all rural development, is the fundamental cause of the present unpromising state of British agriculture, which is affecting adversely the prosperity and security of the whole nation.

One drawback to the English estate system is the size and character of the home farm. It is in but few cases that this has been conducted on business lines, and it has, therefore, proved unconvincing to the tenant farmers. Even where a high standard of live-stock or of cultivation has been obtained, the working farmer has assumed that such methods are uneconomic, and therefore unworthy of imitation by one who has 'to make a living out of his farm.' In this respect Germany has shown a pleasing contrast. The great estate of the typical East Prussian landowner was only in part farmed by his small tenants. He himself had a large demesne. With the agrarian revival, which dated from about 1870, these owners commenced farming their demesnes more intensively instead of finding more estate tenants. They realised the importance of the application of science to farming, and sought skilled scientific managers, and obtained them from the German agricultural colleges, notably from Bonn. This developed a valuable organisation, under which the well-trained young agriculturist could obtain his practical experience as an under-manager before he was selected to control the business of farming commercially a large area of land. In this country, where for many years science and practice have, in spite of the motto of the Royal Agricultural Society of England, existed largely in separate watertight compartments, with a tendency on the part of many of the more influential landowners (at their rent audit dinners and on other like occasions) to disparage the value of the former and evoke applause by so doing, such a development has been impracticable. If, for instance, at the present time an English landowner proposed to farm his 5,000 or 10,000 acres as an industrial undertaking, he would have considerable difficulty in finding a trustworthy manager fully equipped for the post. Every year for many years past suitable men have been leaving the agricultural colleges, but they have found it impossible to obtain the necessary practical experience for the full commercial utilisation of their scientific equipment, owing to their inability to enter the business of industrial farming in a subordinate capacity. Experience on a single farm of average size does not fit a potentially capable man for the management of a big, highly organised farming business. He has not developed the right outlook.

A good illustration of the weakness of ownership detached from occupation when the landlord ceases to have the necessary knowledge and experience may be seen in Italy at the present time. The metayer

system, which is many centuries old, has of late years been breaking down, and the War has accelerated its downfall. The landowner, who was originally its creator and the main source of its stability, was too far detached from the soil to know how the system from time to time had to be modified. The result was that his tenants revolted, and have in many cases obtained for themselves conditions of tenancy to which they were not properly entitled, on the strength of their temporary prosperity resulting from the War. Throughout Eastern Europe, too, and particularly in Rumania and Czecho-Slovakia, there has been a land revolution. Great estates have been forfeited and their land subdivided, solely because the owners have not during the last generation been active participators in the work of production.

The absentee landlord, a *rara avis* in all Continental countries except Italy, is another example of the baneful effects of his non-industrial character in this country. Oblivious of the true meaning of 'manor'[1] and 'mansion,' he often separates himself entirely, not merely from the industry, but from the locality, in which he comes to be regarded as an unsympathetic stranger.

The plight of the Irish landowner, and indeed of Ireland itself, might to-day be far less serious if, during the century prior to the last fifty years, the landowners of that unfortunate country had as a class made their homes amongst the rural population and identified themselves closely with their industrial welfare.

It has come, perhaps unfortunately, to be assumed in this country that there are three classes or sections of the agricultural community, whose interests are distinct and largely divergent, and for whose participation in the proceeds of the industry separate provision must be made. But the divorce of landownership from land cultivation is unnatural; it is not to be found universally prevalent in other countries, nor indeed has it always existed in our own. Its very existence is a deterrent to the full industrial development of agricultural land. It may be (and it is unfortunately the case to-day among many new occupying-owners) that the producer's monetary resources do not suffice to provide him both with the land itself and with adequate capital for the business of farming it. If, therefore, he can obtain his land, buildings and permanent equipment at a moderate rent, representing to the owner only 2 or 3 per cent. on his capital, and without the added burden of maintenance and repairs, it is undoubtedly attractive as a commercial proposition. But whatever provision may be made by the Legislature for securing to the cultivator fair compensation as the reward of his enterprise, the latter must necessarily be restricted in respect of the full development of the property of another, and an adequate return for such full development (even if wise and prudent) can never be provided for by the State without imposing upon the owner of the land a prospective financial burden admittedly too heavy for him to bear or too risky for him to face. In fact, the unification of the rôles of the landowner and farm tenant is a condition precedent to the full, confident and enterprising development of the agricultural industry on economic lines. Moreover, although eighteenth-century economists laid stress

[1] From Latin *manere*, to remain.

upon the increase of rents as a factor in the enhancement of agricultural prosperity, it cannot be gainsaid, as an abstract economic truth, that an increase in the productivity of agricultural land as the result of the producer's enterprise redounds ultimately to the benefit of its owner or his successor. Also, the difficulties of 'tenant right' inherent in the relation of landlord and tenant are apt to increase in direct ratio with a tenant's enterprise exercised on another's land, and must almost inevitably eventuate in dual ownership and the domestic antagonism of agricultural interests.

Great, however, as are the advantages of occupying ownership to the nation and to the industry, it must be recognised that such a system, although capable of wide extension, cannot exist in this country to the entire exclusion of that of landlord and tenant, nor is it desirable. Some of the most skilled, progressive, and deservedly influential farmers in Great Britain are, and will continue to be, farm tenants. Their knowledge of their holdings and their productive capacity is a valuable asset, and promotes output and economy of administration. Although many men of this type have, under the pressure of circumstances, recently purchased their farms, the majority have not. Even if their farms were purchasable the diversion to land purchase of capital usefully employed in its full exploitation would probably be imprudent and uneconomic. From these men their landlords can learn much; with such men they should strive to establish a relationship of friendly and mutually trustful co-operation in all measures which make for the enhanced prosperity of the farmers' business on the estate or in the district. They should also seek to create and maintain a similar *entente cordiale* between the various sectional organisations of the agricultural community wherever these exist locally. On the other hand, where the tenant is an obviously inefficient farmer, depreciating his landlord's property, and steadily impoverishing himself and his family, the landlord, with the moral backing of his more efficient tenants and of the whole local working population, should boldly assume the responsibility of terminating his occupancy. The fact that County Agricultural Committees are now charged with the statutory duty of assisting landlords in this process should accelerate the dispossession of the industrially incompetent. The Agricultural Holdings Acts, while excellent in theory, have in practice operated to afford security of tenure to the bad tenant equally with the good, and have thereby tended to lower the standard of husbandry throughout England and Wales. The standpoint of the public welfare evolved by war conditions has created, fortunately, a saner outlook upon such matters, even among politicians. The trend of legislation has been in the past, and is even now, all against the active landlord. The tide, however, will assuredly turn when he makes it evident that his welfare and that of the State are identical.

The land is unsparing of her faithful devotees. So multifarious are the daily pre-occupations of the successful arable farmer, involving constant personal attention to detail and a readiness to meet unforeseen contingencies, that he can seldom devote time and attention to the work of organisation of the industry and of those engaged in it. This imposes all the deeper obligation upon the more leisured and probably

more highly educated landowner who is not himself a farmer to take his full share in the execution of this indispensable task. In fact, the landowner's duty as an organiser increases in inverse ratio with his activity as an actual producer.

No agent, however competent, can fully discharge the whole duties of the agricultural landowner. Still less can one who is incompetent, whose training is defective, or whose vision is myopic. Just as in Switzerland, where the conservation of forests is essential for the check of avalanches, the State compels a landowner to employ a State-trained forester for the management of his woodlands, so it might be in the national interest here to enact that either the landowner himself or the agent or factor whom he employs shall have passed some test of efficiency as an estate manager.

English law and custom in relation both to the settlement of estates and to the letting of farms have obstructed the full utility of the English landlord as a producer and as an agricultural organiser. The Settled Land Acts, while in the public interest extending the power of a tenant for life under a strict settlement to sell the whole or part of a settled estate, have provided that all moneys realised by such alienation shall be held by trustees and applied for certain purposes only (for the assumed benefit of the inheritance), specifically prescribed either by the settlement or by these Statutes. They do not admit of the proceeds of sale of a part of a settled estate being applied to capitalise farming operations conducted on strictly commercial lines on another part of it. If, subject to proper safeguards concerning the capacity and trained experience of the life tenant, the Settled Land Acts could be amended in this direction, a considerable impetus would be given to farming enterprise on the part of limited owners, especially those who have no monetary resources outside their estates. Had such enterprise been thus stimulated in the past, the capital value of many an estate passing to a subsequent life tenant or to a remainderman would have been not merely maintained, but greatly enhanced—as was that of Coke of Norfolk—and the true object of the settlement would have been achieved, with immeasurable benefit to the nation at large. The Law of Property Act, 1922, while possibly aiding agricultural enterprise by the destruction of copyholds and customary tenures with their fines, heriots, and other feudal dues, may, by the abolition of primogeniture on an intestacy, stimulate and intensify the desire of many landowners to execute strict settlements, and thereby, in the absence of a fresh statutory extension of the powers of a limited owner, augment the difficulties of their successors in the direction of industrial development. The settlement of landed estates has become so serious a hindrance to their industrial (including their agricultural) development by their owners that it is highly questionable whether legislation may not be desirable forbidding the process altogether as being contrary alike to public policy and to private advantage. The heavy burden of recurrent death duties tends in any case to diminish, and possibly to neutralise entirely, the effect of a transaction designed to ensure the continuous devolution of an unimpaired heritage.

Similarly, the old forms of covenant in farm agreements, which

date from the time when the owner had himself considerable knowledge of the industry and its economic possibilities, represented a higher standard of farming than the tenant would naturally adopt if left to his own uncontrolled inclinations. They have been the subject from time to time of well-merited criticism and of legislative interference on the part of the State, because they became harmful to the industry in consequence of their crystallisation by lawyers and their preservation when the conditions had changed. It was not so much the stringency of these farm agreements, but their lack of modification and adaptation, in view of the opening up of new markets and the development of fresh means of land fertilisation, which laid them open to censure. It was, in fact, ignorance arising from the owner's increasing detachment from the processes of agricultural production which, by stereotyping the conditions of his farm tenancies, retarded the enterprise of his tenants and degraded the standard of their husbandry.

It may be suggested that present-day advocacy of the economic activities of landowners, although they be admittedly beneficial to the commonwealth, is inopportune in view of the growing impoverishment by taxation of the landowning class and the sub-division of many large estates which might have proved good units for effective industrial organisation. On the other hand, it may be urged that the sale by many landowners possessing small commercial experience or aptitude of portions of their estates, and the investment of the proceeds in joint-stock industrial undertakings yielding at least twice the amount of their rent, has brought home to the minds of many of them that mere rent-receiving proprietorship was not good business, and that by the industrial or commercial development of their land more wealth was to be won for themselves and their families. Moreover, the sale or subdivision of estates has added to the landowning class many men possessing not merely great wealth but business acumen and wide commercial knowledge, some at least of whom are able to realise the unprofitableness of undeveloped land, or the political unwisdom of land-ownership detached from industry, and have acted accordingly. Conspicuous among these are men of the type of the late Lord Manton, who with great foresight and public spirit have applied their surplus wealth to the conduct of research, and to the personal application of scientific discovery to the daily requirements of agricultural industry. There are, however, unfortunately all too many of those who have embarked in the purchase of landed estates wealth derived from urban industries or from mining who are not prepared to employ in their development those business methods which have led in the past to their enrichment. They are the rather prone to treat their properties as playgrounds, or as instruments for the enhancement of their social position.

The process of territorial disintegration has largely augmented the number of those who combine within themselves the rôles of occupier and owner—the functions of rent producer and rent receiver. The number of agricultural landowners has thus been at least doubled in several counties by recruits from the ranks of the tenant farmers, and unless compelled by the current fall in prices to sell their

properties, these new proprietors are likely to afford an appreciable accession of political stability to the whole landowning section of the community. On the other hand, many country squires, in face of financial stringency and even of domestic discomfort, have been estopped by the ties of family sentiment and tradition from seeking, by the alienation of their ancestral domains, a short cut to material prosperity or enhanced comfort. In such cases desirable estate improvements and sometimes necessary repairs have had to be abandoned, and eleemosynary gifts reduced to a minimum, causing thereby much heart-burning and compunction. How much better it would be, assuming that the estate is not subject to a strict settlement, rendering such a process *ultra vires*, if part of the estate were sold in order to provide the necessary capital for the cultivation or industrial equipment of the remainder of it!

It is here material to consider more closely to what extent the land-owner in Continental countries has been instrumental in advancing the prosperity of agricultural industry, economically, politically, and socially.

On the Continent, speaking generally, the landowner had to derive his livelihood from his land, and in a large measure from its actual cultivation. Landowners with large invested funds were relatively scarce, and there was not any large influx of rich manufacturers whose ambition it was to acquire such power and social distinction as might be deemed to flow from territorial possessions. The Continental land-owner was generally forced to regard his occupation as landowner as the main business of his life, a business requiring proper training, a business to be steadily developed, and just as steadily maintained, as any commercial undertaking. From this personal and individual stand-point arose his sound and intelligent attitude towards the whole rural industry. He realised that if his own individual business was to achieve the maximum of success, the industry of which it was a part must be as highly organised as any other industry in the country.

Speaking generally, in all Continental countries (whether they have a definite agrarian party or not) the political power enjoyed by agriculture is founded on the fact that agriculture is an organised industry. In Great Britain it is not. Foreign agriculturists realised that the effective and complete organisation of their industry was the surest path to political power, and in every case landowners became the leaders in this movement. Although in different countries its details may have varied, its underlying principle was the same. The great incentive to this development of agricultural organisation was the competition of the new world. On the Continent such competition was strenuously fought, and the aid of science was invoked in the contest. In Great Britain the same competition was not effectively met because organisation was wanting, and the landowner failed in the duty of leadership. As he reduced his rents, so the tenant-farmer reduced the labour bestowed upon the land, and reduced, instead of augmenting, what he put back into the land with a view to its yielding an economic return. Agriculturists and Governments were alike to blame.

In Denmark sixty years ago the landowners co-operated with the

clergy in improving the land and in organising rural industry. Far-sighted landowners realised that the day of the big estate was past, and they joined forces with their Government to substitute, with all proper safeguards for economic success, the occupying-owner for the farm tenant. The success has been undoubted, and neither could the landowner complain of unfair treatment nor the tenant of having imposed upon him an undue financial burden. When in recent years force of circumstances compelled the disintegration of the great estates in England, many farm tenants had no option but to purchase their holdings, and the purchase was made under the worst possible conditions. The system of land banks on the Continental model would have simplified the process of such transfer, and would have obviated in a large measure its inevitable risks. In Denmark the landowners have been the pioneers of all new methods and processes in farming, and the farmers in their neighbourhood have followed their example. The fact that the standard of Danish farming is to-day very high and very level is mainly due to the Danish landowners. In the actual work of the co-operative movement the clergy in Denmark, as in Belgium, have played an important part, often acting as secretaries to the co-operative societies, and by precept and example guiding the industrial activities of the smaller cultivators. In strong contrast the English rural clergy are relatively valueless from an economic standpoint, and thus lose much of the personal influence which they might otherwise possess. This was not always the case in English history. In the fourteenth and fifteenth centuries the monks in England were resident landowners, and initiated most of the improvements which were made in the practices of medieval farming. It was their influence which was mainly instrumental in the improvement of live-stock, drainage, reclamation, and the construction of roads and bridges. Further, the Danish landowner, in common with the best types of his class in all Western European countries, because he applies business methods to the cultivation of his land, has been the means of increasing the aggregate yield from the soil of his country for the benefit of the nation, while as a reward for himself he has derived a profit from the process which surpasses what is generally conceived as possible throughout this country. Not only is his estate administered on the soundest commercial lines, and made to yield a fair return to him as proprietor, but because a considerable proportion, and often the whole, of it is farmed according to up-to-date methods he receives a large profit as a cultivator.

Denmark, however, it must be remembered, is a purely agricultural country. An even better example for British comparison is Belgium, because that country possesses an industrial development similar to but even more intensive than that of the United Kingdom. Although its factory output is greater per head of population than in this country, its rural development has been considerable and progressive. The landowners have been pioneers in this work, while the priests have co-operated with a knowledge and enthusiasm unsurpassed in any other country. The result has been that poor and waste land has been brought into high productivity, and Belgium, in spite of her urban

developments, has excelled all countries of the world in her production per acre of cultivated land. The landowners have taken a specially active part in agricultural production of every description as well as in stock breeding and dairying on their estates, and by championing the interests of the rural community in the agricultural societies and in the National Legislature. Many of them insist upon their sons making a specialised study of agriculture, and a considerable number of landowners' wives are beginning to take an active interest in women's institutes (Cercles de Fermières) and in the Institut Ménager Agricole of Laeken, in which prospective landowners' wives are properly trained to play an active part in the social life of the countryside. So well have the whole agricultural community, led by the landowners, performed their part that politicians and the general public alike in Belgium recognise that the welfare of their country depends ultimately upon a flourishing agricultural industry. Not only has considerable attention been paid to agricultural education in all grades of Belgian schools, but a certain modicum of instruction concerning the land and the national importance of its proper development is, even in the urban schools, inculcated in the minds of all future Belgian citizens, with the result that there exists throughout Belgium a sound public opinion in relation to agricultural problems. No such public opinion can be said to exist at the present time in this country. Our landowners are not as a class educational enthusiasts.

In Germany also, which contains to a large extent an urban and industrial population, the Government has concentrated much attention upon the proper development of land and of agriculture. From the political point of view agriculture in Germany, as represented by the agrarian party, is probably stronger in proportion to its urban population than in any other country. It is, however, significant to note that there agricultural organisation for industrial needs preceded its organisation for political purposes. There, too, prior to the War, the great landowners took the lead, and although in some parts of Germany the larger agricultural estates are administered more or less upon English lines, the owner is almost invariably also a farmer, who conducts his farming operations on a strictly business footing. The first step in the organisation of Germany's agricultural industry may be said to have been taken when, in the latter part of the eighteenth century, the landowners founded the Landschaft as a means of providing credit for estate purposes, recognising, as they did, that credit is the life-blood of the industry, if available on easy and attractive terms, but that in the form of a permanent mortgage it is apt to become a burden upon owner and occupier alike. Out of this landowners' bank, which in 1914 had a capital of 150,000,000*l.*, grew the provision of credit for current agricultural needs through the medium of the Raiffeisen and Schulze-Delitzsch Banks, the former of which had a turnover in 1914 of over 300,000,000*l.*

In France most of the large landowners reside on their estates, which they cultivate themselves, either wholly or in part. They take a practical interest in all matters relating to the progress of agriculture, and are everywhere the promoters of co-operation in all its forms. In particular they are usually at the head of the important agricultural

syndicates. Their influence is, however, essentially local, attached to the land, they concern themselves for the most part only with the interests of the population over which their influence directly extends. Very often a large landowner is the maire of the commune, or a member of the Arrondissement Council, or of the General Council of the Department, but very rarely is he a Senator or Deputy, as such positions necessitate prolonged periods of residence in Paris. The influence of the large landowner is specially felt by the 'metayers' in those regions where metayage exists. This influence takes the form of the choice of their live-stock and fertilisers, and of advice as to the methods of cultivation. This is only possible where mutual confidence and friendly relations exist between the landowner and the 'metayers'; in districts where these relations are disappearing or weakening metayage tends to give place to rent-paying tenancy.

In Italy, in those regions where large estates are the rule, the landlord, usually an absentee, often lets his land to intermediaries, who are mere speculators, and who cultivate it extensively with a view to their personal profit without regard to the interests of the community. Elsewhere the landowners, where they themselves undertake the cultivation of their own properties, usually seek to introduce increasingly scientific methods of cultivation, and to draw advantage, in their own interests and that of the public, from the latest teachings of chemistry, biology, and agricultural mechanics. Even on the estates cultivated on the metayer system the landlords, on whom falls the management of the farms, have sought in the past to introduce all such improvements as will increase the yield of the land and improve the economic conditions of the metayers and their families. Latterly, however, the relations between the landowners and the peasantry have, as already mentioned, become somewhat strained; as the demands of the peasants threatened in many cases to exceed the limits of the productivity of the farms the landowners have felt themselves compelled to combine in association for the defence of their own proprietary interests. A close network of such associations has been formed, and these are affiliated to the General Confederation of Agriculture. This organisation, acting on behalf of its affiliated associations, proposes not only to safeguard the interests of the landowning class, but also to carry on propaganda in favour of the technical progress of agriculture and for the betterment of the conditions of the rural classes in general. Recently there has also been formed an agricultural political party, to promote in Parliament the interests of agriculture.

The history of agriculture in the United Kingdom for the last seventy years does not redound to the credit either of landowners or of statesmen. The landowners, who should have given a lead to the industry, failed to do so, largely because they have not as a class been trained for their proper profession, and because in a greater or less degree they have regarded the land as an amenity, but never as a great national problem for the solution of which they were themselves primarily responsible.

The British landowner, if he farms at all, being untrained to the task, often farms indifferently, and generally at a loss. If

he produces live-stock of special merit it is largely for the foreign and not for the home market. Often his farming operations are based upon his ambition to gain public distinction by excelling as a professional exhibitor of prize stock at the leading agricultural shows, without any effort on his part to make such stock a medium for the improvement of the ordinary commercial stock of the country, or even of his own locality. One result of this is a marked and growing gap between the finest British live-stock, which may be reckoned as the best in the world, and the average live-stock of the ordinary commercial farmer, which is probably lagging behind the average standard now attained in many Continental countries.

Although in soil and climate the land of Great Britain can compare favourably with most of the cultivated land on the Continent, the Continental landowner derives as a rule a net income of from 3*l*. to 4*l*. per acre, as compared with 1*l*. per acre in the United Kingdom. Moreover, the Continental landowner so manages his woodlands that they yield, generally speaking, an annual average net profit at least equal to the rental of the agricultural land, and often very much more.

Sir James Caird (the advocate of more liberal covenants in tenancy agreements) more than sixty years ago sounded the trumpet of warning in relation to the threatening decadence of British agriculture, which, however, passed unheeded by the bulk of those best able to profit by and act upon it.[2] England's period of greatest agricultural depression, which followed twenty years later, synchronised with that of Germany's greatest agricultural enterprise. From that time the latter's agricultural progress, based on ascertained knowledge widely and wisely diffused, was steady and continuous. Germany's food-weapons during the late War were at least as deadly as her military weapons, and the fact that the former did not ultimately triumph cannot be placed to the credit of British landlordism. *Fas est et ab hoste doceri.* Owing to lack of enterprise and to the non-utilisation of scientific discovery the number of persons fed from 100 acres of cultivated land in Great Britain prior to the War fell far short of those fed from the same area in Germany,[3] while the average crop yields of Great Britain have for a generation been below those of Belgium and Denmark, although none of the three can boast of a soil and climate more conducive to agricultural productivity. The same British acreage could well be made to produce at least twice the present output of human and animal food. That England should have 55 per cent. of her cultivable land under pasture as compared with only 18 per cent. in Germany is not creditable to the former. In Germany the occupier, if he is not also the owner, demands and enjoys the benefits of a long lease. Moreover, game preserving there is on a relatively small scale, and subservient to the paramount claims of

[2] It is singular to note that Caird, in the preface to his exhaustive survey of British farming, selects for special emphasis two defects, (1) the lack of landowners' initiative, and (2) the non-utilisation of sewage in promoting fertility. There is still room for land improvement from both sources.

[3] 'The Recent Development of German Agriculture,' by Sir Thomas Middleton [Cd. 8305], 1916.

food production. Here the claims of property as such have overridden those of industry, which alone can in the last resort justify property and at the same time enhance its value. The almost pathetic cry on the part of so many landowners of ' Property, property, property ' is a significant indication of the at least temporary decay of the squire of former days, who, although perhaps a feudal autocrat, was an integral part of the industrial machine, and was recognised and respected as such by the other parts.

And yet only sixty years ago English landowners were still the acknowledged pioneers of agricultural improvement! The whole continent of Europe—including especially France, Germany, and Switzerland—were the confessed imitators of English agricultural methods as initiated and perfected by ' Turnip Townshend,' Coke of Norfolk, Lord Somerville, and the Dukes of Bedford. In France De Saussure, in Germany Thaer, and later Stockhardt (a disciple of Sir John Bennet Lawes), and in Switzerland Von Fellenberg, had preached the advantages of English methods, particularly in the matter of crop rotation. The name of the Squire of Rothamsted was a household word throughout rural Europe, and was stimulating more scientific treatment of the soil, while his own bucolic fellow-countrymen, mostly blind to his genius and to their own advantage, were sinking into a condition of static somnolence and smug contentment with the progress of the past. The Germans especially, unlike ourselves, thoroughly believed in the advantages of education and research, and their farmers, unlike ours, greedily absorbed the teachings of science as applied to agricultural processes, notably in the economic employment of feeding-stuffs and fertilisers.

The present-day poverty of the landowning class will, no doubt, be urged, perhaps with some justification, in opposition to their adoption of the rôle which I submit is properly theirs, and which is not capable of vicarious fulfilment, either by the State or by any agent or tenant. Their very impecuniosity, however, may best provide the much-needed driving power, especially if it be associated with knowledge. Coke of Norfolk derived his stimulus from the refusal of a farm tenant to pay what he considered an economic rent. He could boast eventually of having increased his estate income tenfold. His tenants applauded his enterprise and copied his methods. The increase of his rents, reflecting as it did increased national wealth, was even recognised by economists and statesmen as beneficial alike to the agricultural industry and to the State. Some of his improvements no doubt needed initial capital outlay, and this many a modern landlord may be powerless to provide. But co-operation has proved to be to a large extent a substitute for capital in those countries which have most developed their agricultural prosperity, and become our most formidable competitors, even in our own markets. To co-operative methods agricultural landowners must turn to promote the enhanced well-being of themselves and the whole rural community. Moreover, by the establishment of a system, not of State-imposed minimum wages but of friendly co-partnership, profit-sharing and practical human sympathy, untarnished by patronage, and coupled with greater simplicity of living, they must identify and unify

their material interests with those of the rural employees upon their estates. Thus, and only thus, will the economic and perhaps, too, the political solidarity of presently diverse agricultural interests be established, which can best promote on a permanent basis the maximum prosperity of British agriculture.

The trained capacity to produce should be part of the equipment of every agricultural landowner. But still more important for the modern landowner, if he is to achieve his maximum utility, is the capacity to organise. Without it he will never become a true leader, and British agriculture will become a prey to hostile competition from abroad and successful exploitation at home.

The following are some of the methods by the adoption of which British agriculture, under the enlightened direction of trained, far-sighted, and progressive landowners, might, in spite of the competition of countries where labour is cheaper and taxation lower, be stabilised on a remunerative basis:—

The organisation of credit facilities.

The co-operative purchase in bulk of farm requisites and the co-operative sale and distribution of farm produce.

The utilisation of mechanical energy on the farm by means of tractors, electric motors, oil-engines, potato diggers and planters and other labour-saving devices.

The utilisation of water-power for generating electricity and the employment of the latter for driving farm machinery.

The grinding of every variety of corn (including beans and peas) and the substitution of concentrated foods grown on the estate for purchased milling offals, cattle cakes and meals.

The mechanical mixing of foods for live-stock, and their conveyance without handling into mangers and troughs.

The erection of silos, and the ensilage therein of bulky leguminous crops, as well as of oats, ryegrass, and maize.

The utilisation of liquid manure from farm buildings after collection in tanks.

The elimination of scrub bulls and the provision in every locality of live-stock sires of outstanding quality and good parentage.

The establishment of central dairies and bacon factories either for a single estate or for a larger area.

The utilisation of all whey from cheese factories in feeding pigs or by conversion into lactose or lactalbumin.

The preservation of milk or whey in times of glut by desiccation.

The centralised manufacture of concrete for farm and estate buildings, and of lime and ground limestone for mortar and land dressings.

The organised collection of orchard fruit, and its grading, packing, consignment, and retail sale, or its conversion into cider with portable cider-making plant, or in properly equipped central factories.

The pulping of fruit and making of jam.

The preservation of fruit and vegetables by bottling, canning, and desiccation.

The organised collection and preservation of eggs in the spring and

summer, to place on the market in the late autumn and winter, when their commercial value is highest.

The co-operative use of motor-lorries for carrying farm produce to populous centres of distribution.

The co-operative ownership of portable timber-felling and centralised timber-seasoning plant.

The conversion of the timber of one or (by joint ownership of plant) of several estates into planks, barrels, gates, fencing, mattock handles, clogs, &c., and its preservation by creosote or other preservative.

The organisation of the cultivation of sugar-beet, and its conversion into beet-sugar, alcohol, and cattle foods.

The establishment of co-operative central markets, auction marts, and slaughter-houses.

The organisation of comprehensive schemes of local drainage.

The use of draining machines for excavating drains and laying drain-pipes.

The utilisation of village sewage in the production of osiers, and their conversion into baskets.

The erection of centralised waste-product plants for the utilisation as pig and poultry foods of animal carcases of low commercial value.

The organisation of periodical pilgrimages of local farmers to centres of research and demonstration, or to skilfully worked and wisely equipped farms.

And, above all, the elimination of superfluous and unnecessary middlemen.

There is probably no worse consequence of the lack of cohesion, organisation, and leadership in British agriculture than the extent and power of the middleman interest—unparalleled elsewhere in the civilised world—whose parasitic tentacles have slowly yet surely fastened themselves upon the industry and are sucking out its life's blood to the detriment of producer and consumer alike. It is largely a 'horizontal' interest of useless speculators, and not a 'vertical' interest of helpful distributors. While it thrives the industry decays. Where it is itself sufficiently organised it has even been known to dictate imperiously the price of some essential farm product to producer and consumer alike—a price which would have left no margin of profit to the former—and thereby to compel Government intervention in order to avoid helpless acquiescence, a dangerous departure and indicative of the inherent weakness of the industry.

Apart from the heavy burden of local and Imperial taxation, the toll levied by the middleman is the main cause of the poverty-stricken condition of the English agricultural labourer. While companies whose main object and justification are the distribution of British agricultural produce are paying dividends of 25 per cent. or more, or issuing bonus shares to their urban shareholders and to those who 'toil not, neither do they spin,' the countryside is being slowly denuded of its physically and mentally robust manhood owing to the indigence of the agricultural producer, their emigration is being fostered by statutory enactment, and foreign produce of the same or a like description is being sold in increasing quantities in British markets. It is an unedifying spectacle which

agricultural solidarity and leadership alone can efface. In no sphere of action can the leadership of the landowner be more profitably exercised; in none is it more urgently needed.

The disparity between the prices paid to the farmer for his produce and those paid by the consumer for the same produce, or even by the farmer himself for its by-products, may be illustrated by the following official figures furnished to me by the Statistical Branch of the National Farmers' Union:—

WHEAT AND ITS PRODUCTS.*

Per Ton.

—	Wheat	Coarse Middlings	Bran	Flour
	£ s. d.	£ s. d.	£ s. d.	£ s. d.
Average pre-War ...	7 12 5 (1911–13)	6 12 0 (1911–13)	5 1 0 (1911–13)	11 0 0 (1913)
1921				
July	19 13 10	12 0 0	8 3 0	27 8 0
August	15 17 8	14 2 0	10 0 0	26 4 0
September	13 13 9	12 13 0	9 1 0	24 16 0
October	11 19 7	11 2 0	8 8 0	23 4 0
November	10 10 0	10 16 0	8 17 0	20 12 0
December	10 13 2	10 14 0	10 1 0	18 16 0
1922				
January	10 11 2	9 2 0	9 7 0	18 4 0
February	11 1 9	8 18 0	9 1 0	19 4 0
March	12 6 2	8 13 0	8 11 0	20 8 0
April	12 0 0	8 6 0	8 1 0	20 4 0
May	12 18 3	8 19 0	8 0 0	19 14 0
June	12 13 10	8 13 0	6 13 0	18 12 0

* The price of wheat is based on the monthly average *Gazette* price published by the Ministry of Agriculture.

The prices of English offals are those for the two varieties quoted officially by the Ministry of Agriculture.

The price of flour is the average of the prices at the beginning and end of the month for London Straights quoted by *The Times* and incorporated in its index-number of prices.

Expressing these prices as index-numbers, with the average pre-War prices quoted above taken as 100 in each case:—

—	Wheat	Coarse Middlings	Bran	Flour
1921				
July	258	182	161	249
August	208	214	198	238
September	180	192	187	225
October	157	168	166	211
November	138	**164**	175	187
December	140	**162**	199	171
1922				
January	138	138	185	165
February	145	135	179	175
March	161	131	169	185
April	157	126	159	184
May	169	136	158	179
June	167	131	132	169

The most striking comparison of prices is provided by the months November and December 1921. In both these months **the price of coarse middlings was actually higher than the price of wheat.** The percentage increases on the pre-War levels are also instructive, viz.:—

—	Percentage Increase	
	Nov.	Dec.
English wheat	38	40
,, coarse middlings ...	64	62
,, bran	75	99
Straight-run flour	87	71

Taking the respective food values of wheat, coarse middlings, and bran as represented by the figures 100, 85, and 65 respectively, the relative values to the pig-feeder, apart from dietetic considerations, would be as follows:—

—	Wheat			Coarse Middlings			Bran		
	£	*s.*	*d.*	£	*s.*	*d.*	£	*s.*	*d.*
Average pre-War...	7	12	5	6	9	7	4	19	1
Nov. 1921	10	10	0	8	18	6	6	16	6
Dec. 1921	10	13	2	9	1	2	6	18	7

MILK.

Summer Contracts, 1922 (London Supply).

Per Gallon.

	To Farmer	To Consumer
Prices originally proposed and agreed to (*re* ¾ of supply)	8d. delivered London, carriage paid,	1/8
Equivalent to (average)	6d. at farm	
Prices after Government intervention	10½d. delivered London	1/8
Equivalent to (average)	8½d. at farm	

PIGS AND BACON.

Pigs.

Per Lb. deadweight (in pence).

—	1st quality	2nd quality	Average	Index-number of average
	d.	*d.*	*d.*	*d.*
*Average 1911–13 ...	6.4	6.0	6.2	100
1921				
July	14.3	12.9	13.6	219
August	14.8	13.6	14.2	229
September	14.1	12.8	13.5	218
October	12.0	10.9	11.4	184
November...	11.0	9.7	10.4	168
December	10.3	9.1	9.7	156
1922				
January	10.3	9.1	9.7	156
February	11.4	10.2	10.8	174
March	11.9	10.7	11.3	182
April	12.1	11.0	11.6	187
May	12.3	10.9	11.6	187
June	11.7	10.5	11.1	179

* Average prices of bacon pigs as quoted officially by the Ministry of Agriculture.

Retail Bacon Prices.†

—	Back per lb.	Side per lb.	Average per lb.
	s. d.	*s. d.*	*s. d.*
Pre-War (1911–13) ...	1 3½	0 11	1 1½
1921			
July	3 3¼	2 6½	2 10¾
August	3 2	2 5½	2 9¾
September	3 1½	2 4½	2 9
October	2 11	2 1¼	2 6¼
November...	2 5¾	1 9	2 1¾
December	2 6¾	1 10¼	2 2½
1922			
January	2 6½	1 9¾	2 2¼
February	2 6½	1 10	2 2¼
March	2 6½	1 9¾	2 2¼
April	2 6¼	1 10	2 2¼
May	2 7¾	1 11¼	2 3½
June	2 7½	1 11¾	2 3½

† These figures are obtained from five of the largest multiple stores in London.

In the following table these prices are expressed as index-numbers (the pre-War price in each case being taken as 100) and compared with the corresponding index-number of prices of bacon as given above:—

Index-Numbers.

—	Price of Bacon Pigs	Retail Price of Bacon*	
		Back	Side
Pre-War (1911–13) ...	100	100	100
1921			
July	219	253	277
August	229	245	268
September	218	242	259
October	184	226	229
November	168	192	191
December	156	198	202
1922			
January	156	197	198
February	174	197	200
March	182	197	198
April	187	195	200
May	187	205	211
June	179	203	216

* It is noteworthy that the disparity between the pre-War and post-War prices is most marked in the cheaper cuts.

It may be mentioned incidentally that the current retail price of potatoes is at least six times and that of plums at least ten times the price being paid to their producers.

England greatly needs, on the part of those landowners whose material resources admit, the provision of such factory or other equipment as will make agricultural estates to a greater extent self-contained industrial units depending less upon the outside world for the raw materials of the rural industry [4] and for the absorption or conversion of its output.

Such estates personally managed by their owners as business concerns were to be found in many parts of the Continent, notably in Hungary. In Belgium those of Baron Peers at Oostcamp and of the Chevalier de Vrière at Bloemendael, and in France that of the Viscomte Arthur de Chezelle (who introduced ensilage into England) at Le Boulleaume, Oise, may be mentioned as examples deserving of English imitation.

There are probably few directions in which landowners can more usefully employ their salutary influence and organising capacity than in that of finding profitable outlets for the agricultural produce of their estates. As a good illustration of what can usefully be done in this direction may be selected the enterprise of potato-growers in the Wash district of Lincolnshire in catering for the special requirements of the chip-potato trade in the North of England, and of the Evesham market gardeners in satisfying the predilections of Lancashire mill hands in the production of spring onions of a special description and flavour. Both enterprises have resulted in the acquisition by their growers of considerable wealth and prosperity.

[4] M. Terentius Varro (B.C. 36) in his De Re Rusticâ, Lib. I., Cap. XXII., said : 'Quae e fundo sumi non poterunt, ea si empta erunt potius ad utilitatem, quam ob speciem, sumptu fructum non extenuabunt. Eo magis, si inde empta erunt potissimum, ubi ea et bona et proxime et vilissimo sint emi poterunt.'

In all land policy it is difficult to reconcile, especially among a proletariat ignorant alike of economics and business, the social and political aspect of the problem with sound economics, and the former being generally more popular and lending itself to makeshift opportunism is apt to dominate the counsels of Government, to the exclusion of those which may appear hard and unsympathetic, but which are often fraught with a wider and more continuous prosperity to the great masses of the population. Thus it was that the enclosure of the commons, which multiplied exceedingly the output of agricultural wealth, was strenuously resisted in the sixteenth and seventeenth centuries, and only gained its great impetus and development in the latter half of the eighteenth century, when its undoubted advantages had become realised by many of those who most sympathetically championed the interests of the poor. Thus it is to-day with the artificial extension, under strong Government pressure, of statutory small holdings beyond the area of their possible absorption by experienced cultivators of sufficient capital, in the absence of effective co-operation and during a period of falling markets. But social and political prejudices, even when directed against a class which on balance is an asset to the State, must be taken into account in the balancing of economic advantage, and even more so now than in those expansive days when George III. was king, when agricultural landowners were the predominant political force, and when Arthur Young preached his illuminating economic gospel, which, in the practice of his disciples and with the assistance of scientific discovery, carried the agriculture of Britain to its pre-eminent position amongst the nations of the world.

It is often said of social revolutions, as it is being said of the post-War Russian Revolution, that the cause is to be found in the monopoly of land in the hands of a few great landowners. It is at least open to doubt whether this has ever been the main cause of any revolution, and certainly was not so in the case of that which has been recently prevalent in Russia. In 1917, and for many decades previously, the great Russian landowners only owned one-tenth of the land of Russia, the other nine-tenths belonging to the peasants, or rather to their communities. This land was managed by the Communal Council, or ‘ Mir,’ which periodically met to allot land for cultivation to members of the commune, who, as a result, occupied individual holdings, enjoying their use until another re-allotment took place. It is noteworthy, however, that the one-tenth of the nation’s land under the control of the large individual landowners was that upon which the most care was bestowed and the most up-to-date methods were employed, with the result that the output of food from this one-tenth exceeded the total output of the other nine-tenths, which were under the control of the peasant communes, and which were badly cultivated and managed. It was when the Revolution drove out Russian landowners that the production of food decreased so seriously as to threaten the nation with the horrors of starvation.

Whereas a relative paucity of landed proprietors in a populous and preponderantly urban country engenders political antipathy and an unsympathetic Government attitude, a multiplication of small owners

lacking individual initiative and enterprise encourages, and indeed compels, Governmental guidance, interference, and control. In France, for instance (a nation of peasant proprietors), the State to a great extent takes the place and performs the economic functions of the large landowner. But the State can take no risks in developing a commercial enterprise even when science points the way. It may encourage and subsidise scientific investigation, but it cannot compel its application to agricultural practice. In England it was private enterprise which reclaimed wastes, drained marshes, consolidated uneconomic holdings, enclosed commons, and raised at one period the quality of British livestock, and at another the standard of British cultivation, to a position of unchallenged supremacy throughout the world.

The original 'Board of Agriculture,' which was founded in 1793 on the initiative and inspiration of Arthur Young, was for a time the chief agency by which a policy, dictated originally by the enlightened self-interest of the larger landowners and fostered by the demands of a growing manufacturing population, was extended to the public advantage throughout the kingdom. It expired twenty-nine years later, during a period of acute agricultural distress, because it had exhausted its usefulness, and was found to be less efficacious in promoting agricultural development than individual enterprise backed by the employment of individual capital. The Royal Agricultural Society of England, founded in 1838, became its legitimate and acknowledged substitute, and, in fact, marked the revival of rural prosperity which synchronised with the acceptance for a time by landlords of the duties of their position. In every civilised country the necessity for State guidance and State control is in direct ratio with the prevalence of small landowners. This control, while necessitated in France by a peasant proprietary, has there been kept within bounds by the powerful and widely diffused political strength of the agricultural industry. In England, in the absence of such strength, Government control as it extends is bound to be subordinated to urban interests and urban, and often ignorant, prejudices. In a country where the agricultural population are in a small and diminishing minority Government leadership and landowner leadership are mutually incompatible and mutually destructive. The abandonment of the latter by a failure to found power upon the informed exercise of duty must ultimately lead to Land Nationalisation. There is no small danger to an industry involved in its exclusive possession of a separate State Department necessarily swayed by inconstant and incalculable political currents. If some other Department of the State were to take over the administration of animal diseases and of milk control, and assuming that considerations of national economy were to result in the entire abolition of the Ministry of Agriculture, or at least in the limitation of its activities to the organisation of agricultural research, and if simultaneously landowners were to assume enlightened leadership of the industry and the Royal Agricultural Society were to carry out to the full the original intentions of its founders, British agriculture would probably acquire more permanent stability and the nation consequentially enhanced security. Failing the simultaneous and improbable fulfilment of all these conditions, the growing

enterprise of landowners should, in the public interest, obviate the necessity for ever-increasing Government intervention and control.

The long-continued divorce until comparatively recent times of science and agriculture in Great Britain was somewhat remarkable, and accounted to no small extent for the discontinuous progress and prosperity of the latter. The landowner, who, with the dissolution of the monasteries, alone governed the economic destinies of the countryside, was seldom a farmer and never a scientist. His own education fitted him for the profession of arms, court life, sport, politics, or diplomacy. His personal association with industry or commerce would have placed him outside the social pale. It was, it must be admitted, the tenant farmer—and notably Robert Bakewell, of Dishley—who in the Golden Age of agricultural progress was the pioneer of live-stock improvement. But it was the landowner who was the pioneer of improvements in the cultivation and output of the soil. It was, however, as educated thinkers, alive to the economic needs of their times, rather than as agrarian experts, that men like John Evelyn and Sir Richard Weston in the seventeenth century, and Jethro Tull, Charles, second Viscount Townshend, Coke of Norfolk, and the fourth and fifth Dukes of Bedford (the latter the founder of the Smithfield Club) in the eighteenth century, advocated and carried through a veritable revolution in agricultural practice. Jethro Tull, a briefless barrister, was the originator of the horse-hoe, as well as of the drill for sowing wheat and oats. He and Lord Townshend, the statesman, by popularising the cultivation of turnips and of leguminous crops, led to the introduction of the four-course rotation as a normal agricultural practice, and established a definite link between pastoral farming (conducted mainly for the production of wool) and arable husbandry, rendering possible not merely the winter feeding and consequent preservation of live-stock, but also the largely augmented production of bread, corn, meat, and milk. So, too, Thomas Coke, the sportsman, society beau, and politician, by adopting and extending the methods of his Norfolk neighbour, not only multiplied exceedingly the agricultural wealth of a barren tract of country, 'which was little better than a rabbit warren,' and induced his tenants at enhanced rents to copy his methods, but also by making his annual 'sheep shearings' a fashionable rendezvous stimulated many other landowners to follow his example. The progressive and profitable activities of these pioneers were further advertised and contrasted with less enlightened methods both at home and abroad by the brilliant and indefatigable Arthur Young, who 'was not so much instrumental in conveying knowledge to the common farmer as in becoming the vehicle by which the latter's want of knowledge was made known to experts.' [5] The same gospel was subsequently preached by Cobbett and Caird.

None of these great men, whatever may have been their superficial acquaintance with political economy, could be described as scientists. They knew nothing of chemistry, physics, or biology. They were, in fact, mere empiricists. Strangely enough, concurrently with the rapid advances in farming practice science was making giant strides in the direction of assisting the agricultural industry without the knowledge

[5] Russell Garnier's *History of the English Landed Interest*, 1893.

of its participants, and in providing the true explanation of the success of many of their empirical processes. Wallarius, the Swede, about 1760 was demonstrating the value of humus in promoting soil fertility. De Saussure, the Swiss, towards the end of the century was explaining the nutrition of plants and their absorption of carbon from the air and ascribing, somewhat inaccurately, their physical stability to the action of phosphates. Thaer, the German (the Hanoverian physician of George III.), in 1804 was founding the first agricultural college in Europe, and pointing the way to Liebig in his discoveries of the ash constituents of plants. Finally, Boussingault, the Frenchman, about 1820, covering the whole range of agricultural chemistry and testing his theories on his estate at Béchalbronn in Alsace, was bringing his influence to bear directly upon the agriculture both of France and of England, and was affording the chief inspiration to Lawes and Gilbert in the successful conduct of their long and beneficent partnership, especially in the employment of the statistical method in calculating the effect of fertilisers upon the growth of plants. It was not in fact until the time of Boussingault and Lawes, and after Sir Humphry Davy had, with all his great authority as a chemist, given, as it were, his *imprimatur*, that the two separate and converging lines of scientific discovery and agricultural practice may be said to have met, and the two methods—the scientific and the empirical—to have become fused. What Davy, the chemist, foreshadowed, Lawes, the landowner, consummated.

Throughout this period of agricultural enlightenment there were critics of the progressive but not unfashionable industrial tendencies of the landowners of the day. As Lord Ernle recalls in his recent book,[6] Dr. Edwards in 1783 wrote: ‘Gentlemen have no right to be farmers, and their entering upon agriculture to follow it as a business is perhaps a breach of their moral duty.’ Nevertheless, large numbers of young men who were heirs to landed estates, as well as sometimes their younger brothers, began to go as pupils to farmers.

Thus, too, in the earlier days of the eighteenth century the appellation of ‘projectors’ was derisively applied to those enterprising amateur farmers who became the pioneers of modern farming. The adoption of any new system of husbandry, such as Jethro Tull’s turnip drilling, was deprecated (especially in the Northern counties) by the rank and file of the farming community, on the ground that a rent was payable by the farmer to his landlord, and that the adoption of any innovation was consequently accompanied by grave financial risks. It was the dogged persistence of the ‘projectors’ and the obviously remunerative results of their own improved methods which silenced the critics and compelled imitation.

Fashion is an important factor in directing the activities of persons of independent means, and fashion has frequently in the past been dictated by Royal example. Thus in the days of Edward I., who was a gardener, and in those of Edward II., who was a farmer and horse-breeder, there was a temporary and healthy enthusiasm on the part of successive Lords of Berkeley and other great territorial magnates to increase the productiveness of their lands by marling, paring, and

[6] *English Farming, Past and Present*, 3rd edition, 1922.

burning, and such other methods of improvement as were recognised as beneficial in those primitive times. Again, the great revival of agricultural industry during the latter part of the eighteenth century was largely due to the example set by George III., who, under the assumed name of his shepherd, 'Ralph Robinson,' contributed to the monthly publication known as the *Annals of Agriculture*, and who made no secret of the fact that his interest in his farming operations exceeded that afforded him by affairs of state. He revelled in the title of 'Farmer George' and took a deep and personal interest in his flock of merino sheep and his stall-fed oxen. So far as was practicable he turned Windsor Castle into a huge farmhouse, and its grounds into an agricultural holding. His maximum happiness was achieved when comparing notes with a farming neighbour, quoting the dicta of Arthur Young, or personally superintending the drainage or cultivation of his Flemish or his Norfolk farm. Amongst those who followed the Royal example were Lord Rockingham at Wentworth, Lord Egremont at Petworth, and Sir John Sinclair, the President of the first Board of Agriculture. In more recent times the same traditions have been maintained or revived by men of outstanding enthusiasm and vision, such as Philip Pusey, Sir Thomas Acland, Albert Pell, and Lord Rayleigh.

In the main, however, even the more enlightened and progressive landowners have during the last century failed to achieve much for the benefit of the industry through lack of a comprehensive and well-thought-out plan, through discontinuity of effort, or through the consciousness that they were failing to carry complete conviction to those engaged therein as a source of livelihood.

It is worthy of note, and tends to confirm the cynical and trite observation of Swift, that the duplication of a single ear of corn or a single blade of grass 'does more essential service to mankind than the whole race of politicians put together,' that the fame of the second Viscount Townshend, who was Secretary of State under George I. and George II., and subsequently Lord Lieutenant of Ireland and Controller of the Foreign Policy of Great Britain, should have passed down to posterity as that of an agriculturist rather than as that of a statesman. As Arthur Young with prophetic vision says of him: 'The importance of Embassies, Vice-Royalties and Seals is as transitory as that of personal beauty, and the memory of this lord, though a man of great ability, will in a few ages be lost as a Minister and Statesman and preserved only as a farmer.'

It is an interesting fact that while during the eighteenth century landowners like Townshend and Coke were the pioneers of improvements in tillage, and tenant farmers of those in live-stock, the converse has been the case during the last 80 to 100 years. Prominent among farm tenants who in the former period established upon firm foundations various breeds of cattle and of sheep were Bakewell, Charles and Robert Colling, Matthew and George Culley, the Booths of Warlaby, Bates, Benjamin Tompkins, Hewer, Quartly, and Ellman of Glynde. The names of Treadwell, Hobbs, Prout, Dennis, Clare Sewell Read, Jonas Webb, and James Hope of Dunbar may be mentioned among modern farm tenants who maintained a high standard of arable

husbandry (not unassociated with the maintenance of good flocks) during an age when there was relatively little general progress in crop cultivation. Concurrently, however, and especially during the last fifty years, British live-stock of every description has steadily improved and has attained a position of acknowledged superiority throughout the world. This is largely attributable to the stock-breeding enterprise of three successive sovereigns, including King George V., and to the enthusiastic efforts of such other landowners as the late Duke of Richmond and Gordon, Lord Rothschild, Lord Fitzhardinge, Sir Nigel Kingscote, and Sir Walter Gilbey. But the enterprise of landowners in this respect has not, as a rule, been conducted on strictly commercial lines, and has often been dissociated from the nationally more important task of land cultivation.

It is an unfortunate fact which emerges from the annals of the English countryside throughout several centuries that the attainment by the landed proprietor of such a measure of wealth, whether arising from periods of agricultural prosperity or from external sources, as will leave a fair margin over and above the reasonable requirements of family comfort, has produced an inclination to exchange the position of wealth producer for that of rent receiver, and to become progressively detached in activity and interest from agricultural pursuits. Groping after political power, clambering after social elevation, excessive indulgence in sport and the adaptation or sacrifice of landed property to its demands, and the pursuit of careers evoking a stronger appeal to national sentiment or conspicuous achievement, have all operated to detach the owners from the soil. Thoughtful patriots and economists of all ages have commented upon this tendency with regret. 'Our gentry,' writes Pepys during the agricultural depression of the latter part of the seventeenth century, 'are grown ignorant in everything of good husbandry,' and he deplores the fact that without their initiative progress is almost impossible.

John Stuart Mill surely enunciated sound economic truth, as well as wise public policy, when, writing in 1848, he said: 'The reasons which form the justification . . . of property in land are valid only in so far as the proprietor of land is its improver. . . . In no sound theory of private property was it ever contemplated that the proprietor of land should be merely a sinecurist quartered upon it.'

Whenever agriculture is depressed fiscal Protection is sought as the chief remedy for its ills. Dependence upon Government is apt to destroy initiative, self-reliance and resourcefulness, and to breed inertia. It is at best a broken reed upon which to lean in an industrial country with a teeming urban population. If the imminence of threatened starvation in times of war evokes Government measures of artificial stimulation to the process of food production they are necessarily ephemeral and evanescent, and can afford no continuing stability. The prospect of relatively cheap seaborne food is sure to discredit among urban workers any policy which raises artificially the cost of that produced at home or extends its production by subsidies, provided mainly at the expense of the non-agricultural population. German agriculture flourished in pre-War days not in consequence of, but in spite of, its Protectionist policy

It is not by increasing the cost of food, but by decreasing the cost of its production and the State-imposed burdens upon cultivated land that the economic salvation of British agriculture can best be secured. The former course can but reduce demand and antagonise urban interests, while the latter will have the contrary effect.

The British agricultural landowner is to-day on his trial. Unless he justifies himself as such the Nationalisation of the Land is inevitable. Public opinion will demand his extinction, and Parliament will endorse the demand. Most landowners have been for the last two generations mere rent receivers, and have possessed neither the knowledge nor the inclination personally to administer their own estates, still less to cultivate them on commercial lines for their own and the nation's benefit. So far as they have been organised as a class of the community they have been organised, not as producers of wealth, but as defenders of property, and as such their organisation has, in a highly democratic country, afforded them but a small and steadily decreasing measure of security. They have thus lost their political power, because it had no economic basis. As individuals they have, in the main, done good service to the State. No class has consistently shown itself more patriotic, unselfish, and philanthropic, or more imbued with a high sense of public duty, inspired by lofty traditions unrivalled in any other country in the world. As statesmen and as local administrators they have, while occupying the position of the governing class, set a standard of political and commercial integrity which permeated the national life. They have been stigmatised, not wholly without justification, as ignorant, reactionary, and despotic. But at least it can be said that during the period when their power and influence in the State were greatest Britain attained to her outstanding position as the chief democracy of the world, and as the great champion of liberty, alike of person, of speech, and of Press.

Assuming that landowner organisation and landowner leadership as a condition precedent thereto are urgently necessary on the one hand for the welfare of the agricultural industry, and on the other for the greater security of the nation, through the material increase of its food and timber output, there would appear to be two alternative types of landownership, and two only, likely to find justification in post-War Britain, namely, individual proprietorship based upon agricultural training and commercial experience, or the proprietorship of the State, effected through the Nationalisation of the Land. The former alternative is still possible if landowners will but bestir themselves and take upon their shoulders the responsibility which is pre-eminently theirs, and which is incapable of effective delegation or vicarious execution.

The factors which give promise that in the future the British landowner will once more take his proper place in affording an enlightened lead to the agricultural industry, and will thus bring about a rural renaissance comparable to that of 150 years ago, are, on the one hand, his present impoverishment, and on the other his growing desire to be suitably trained for his managerial duties. It was the poverty of the landowner which, in Denmark, Germany and Belgium, created the necessary impetus to agricultural progress in those countries

in the latter half of the nineteenth century. Oxford, Cambridge, and our other universities, as well as the agricultural colleges, are to-day training large numbers of prospective landowners in the science and practice of agriculture—a course which a generation ago would have been deemed vulgarly utilitarian, and inconsistent with the traditions of a liberal education—and many hundreds are flocking to avail themselves of the opportunities thus afforded. Some, too, of our public schools, and notably Repton, Oundle, and Christ's Hospital, alive to the new demand, are including in their curriculum the study of agriculture, while others, averse from early specialisation, are strengthening their science teaching as a prelude to more specialised instruction elsewhere. But such training, wherever acquired, to be really effective must not be that of the mere well-informed onlooker and critic. It must include personal acquaintance with the actual manual processes of husbandry if the rural employer and organiser of the future is to understand fully the daily tasks of the farm worker, his difficulties, his mentality, and his potential output. He should, if practicable, work as a labourer (as does many an enterprising young Danish landowner) for at least a year on a well-conducted and well-organised arable farm, preferably before, and not after, he studies the scientific or even the commercial side of the business. The most efficient education is generally from the concrete to the abstract, rather than the reverse. The lack of commercial training has ruined many a hard-working 'gentleman farmer.' He should learn the rudiments of commerce and not be ashamed to do his own marketing. If possible, too, he should by means of travel learn something of the methods of husbandry practised on the Continent as well as in other parts of the United Kingdom, as did Archbishop Morton (the pioneer of the drainage of the Fens), Hartlib, and Sir Richard Weston in Flanders, Jethro Tull both in Flanders and in France, Viscount Townshend in Hanover and Holland, and Arthur Young throughout France, Great Britain, and Ireland. He will ultimately embark upon his life's work—the pleasantest and most engrossing of all pursuits—with an equipment far exceeding that of Townshend or of Coke. They were empiricists, groping by experiment and often disappointing experience towards the light, without the conscious aid of science. In the landowner of to-day the association of practice with science, and the capacity for leadership inherent in every healthy Briton, should carry him to spheres of successful economic achievement to which they could never have aspired, and concurrently raise the reputation of British farming once again to a pinnacle of undisputed superiority above all its rivals.

A leading land agent, speaking recently at a large gathering of the land agents' profession in London, significantly said: 'Our principals are getting even more difficult to manage than their estates.' Surely this intractability is a sign of grace, an evidence that the landowning fraternity are at last awakening from the irresponsible torpor which has for long benumbed their potential utility.

Perhaps, however, the greatest stimulus to enterprise, born of increased confidence on the part of landowners, will prove to be their consciousness of the numerical reinforcement of the class to which they

belong. The following tables, taken from the most recent official statistics of the Ministry of Agriculture, show the differences between the number of occupying owners and of their holdings in the years 1913 and 1921 respectively:—

Separate Occupations.

Year	Total Number of Holdings	Number of Holdings owned or mainly owned by occupiers	Percentage of Holdings owned
1913	435,677	48,760	11·19%
1921	420,133	70,469	16·77%

Acreage.

Year	Total Area owned by occupiers	Total Area under crops and grass	Proportion of Total Area under crops and grass
	Acres	Acres	
1913	2,891,000	27,129,000	10·7%
1921	5,232,000	26,144,000	20 %

That the occupying owners should have increased during the last eight years by 49 per cent. and the acreage which they occupy by nearly 100 per cent. is indicative of the augmented strength, numerical and geographical, of a class which was once deemed to be the backbone of the nation. If many of the new occupying owners are to secure permanent stability in their present position, it is urgently desirable that the Government should afford them credit on easy terms in order to enable them to discharge gradually and without undue embarrassment the debts outstanding in respect of their recent purchases. The absence in this country of Land Banks similar to those existing for this purpose in several Continental countries is hampering alike to food output and to financial security.

So, too, the long overdue revision of the present system of Local Taxation has become a matter of urgent necessity. A system which dates from a period when real estate was the almost exclusive source of national wealth is indisputably inequitable at a time when, as now, it comprises about one-tenth only of that assessable to income tax, and especially so in the case of agricultural land, which represents less than one-eighth of the total property assessable to local rates, and upon which the burden fails with particular severity, owing to the large area of rateable property required for the purpose of a business yielding a relatively small income (see Appendices I. and II.).

The annual aggregate assessment to income tax in respect of the ownership of land under Schedule A was by a curious coincidence almost identical in the years 1814-15 and 1913-14—namely 37,000,000*l*. (It rose gradually from the former year until it reached its maximum of 52,000,000*l*. in 1879-80) (see Appendix III.).

The capacity of landowners as a class to direct the organisation of agriculture must depend in some measure, as Continental experience demonstrates, upon their capacity to organise themselves. Otherwise

their efforts will be not national in their scope, but isolated and sporadic. In this connection the existence and growing strength of the Central Landowners' Association is a welcome augury of future corporate efficiency. Composed exclusively of agricultural landowners, and rigidly excluding even land agents and professional advisers from its ranks, it already has local branches in all but two of the counties of England and Wales, and is beginning to enter into friendly negotiations with similar sectional organisations of farmers and agricultural workers for the advancement of the interests, both national and local, of the industry as a whole. While primarily a political (although a non-partisan) association, its objects are not merely politically defensive, but to a growing extent economic and constructive. In any agrarian movement in the future it seems likely to play a conspicuous and useful part, and to help in cementing the solidarity of agricultural forces, without which continuous agricultural progress is difficult of attainment.

What is most needed in rural Britain to-day is pride on the part of landowners, great and small, in their class, and a consciousness of their beneficent and reconstructive power, coupled with a stolid determination to play their part—the leading part—with knowledge and sympathy in the building up of a well-organised and mutually helpful agricultural community, undeterred by transient difficulties, and unshaken by the temptation to evade their high responsibilities by the entire alienation of their ancestral estates, or by evoking Government aid in the solution of economic problems which they alone can best solve. Their traditions are great, but their future destiny is greater, if they have but the vision, the courage, and, above all, the will to press resolutely forward towards the goal to which public duty and material advantage alike point the way.

But no policy, however prudent, can gain public approbation and endorsement in the twentieth century which discounts the human factor—which in fact does not, in conformity with Jeremy Bentham's doctrine of 'Utilitarianism,' conduce to 'the greatest happiness of the greatest number.' Upon the prosperity of the industry depends the remuneration of the worker and his access to domestic comforts beyond the bare necessaries of life. Upon it depends the maintenance of the social and recreative side of village life. The disruption of landed estates is often accompanied by social disorganisation of the village community and stagnation of those activities and interests which afford an invigorating alternative to the routine of the wage-earner's toil, and tend to enhance his occupational keenness and efficiency. If, then, the welfare, economic and social, of the rural population rests ultimately upon that of the industry which affords them employment, and if this in turn depends upon the wise leadership of the landowning class, may not the moral 'Utilitarianism' of Bentham be combined with the commercial utilitarianism of the twentieth century, and the decadence of the landowner be deemed to be synonymous with, or at least a prelude to, that of the rural worker? If so, it will not be untrue—but may it never be necessary—(corrupting Goldsmith's famous couplet) to say:—

'Ill fares the land, to hastening ills a prey,
Where wealth accumulates and *squires* decay.'

APPENDIX I.

Extracts from Reports of the Commissioners of His Majesty's Inland Revenue.

Table 105.—Details of the Gross Income from the Ownership of Lands, Houses, etc., the deductions therefrom, and the Income on which Tax was received for the Year 1912-13.

—	England	Scotland	Ireland	United Kingdom
Gross Income :—	£	£	£	£
1. Lands, including Rent-charges under Tithes Commutation Act, Farmhouses, Farm Buildings, etc.	36,813,122	5,730,311	9,694,780	52,238,213
2. Houses, Messuages, Tenements, etc.	199,647,729	20,978,462	5,364,407	225,990,598
3. Other Property :— Manors, Fines, certain Tithes, certain Sporting Rights, etc.	850,234	455,624	1,727	1,307,585
Total Gross Income	237,311,085	27,164,397	15,060,914	279,536,396

Table 65.—Income from the Ownership of Lands, Houses, etc.; Details of the Assessments made in the year 1918-19.

—	England	Scotland	Ireland	United Kingdom
Gross Income brought under the Review of the Department :—	£	£	£	£
*1. Lands, including Rent-charges under Tithes, Commutation Act, Farmhouses, Farm Buildings, etc.	36,700,000	5,580,000	9,700,000	51,980,000
2. Houses, Messuages, Tenements, etc.	207,648,080	21,967,071	5,807,606	235,422,757
3. Other Property :— Manors, Fines, certain Tithes, certain Sporting Rights, etc.	835,000	460,000	1,300	1,296,300
Total Gross Income	245,183,080	28,007,071	15,508,906	288,699,057

* Under this head appears mainly the annual value (inclusive of tithe rent-charges under the Tithe Commutation Act) of farm lands and buildings. In addition to farm lands the heading includes farmhouses occupied by tenant farmers or farm servants, orchards, woodlands, lakes, etc., and any gardens or pleasure grounds held with mansions or houses in excess of one acre adjoining such properties. The value of such gardens or pleasure grounds up to one acre in extent is excluded from this head, and included under the second heading "Houses, etc."; farmhouses of annual value of £20 or upwards not occupied (as above) by tenant farmers or farm bailiffs are also excluded, and appear under head (2) "Houses, etc."

APPENDIX II.

Rateable Property in England and Wales.

(Hansard, V. 151, No. 20, Col. 898.)

Value	April 1920	April 1921
1. Rateable Value of rateable hereditaments :	£	£
i. Agricultural land	24,736,662	25,326,493
ii. Other rateable hereditaments . .	208,590,479	218,762,373
2. Annual Value of non-rateable Government property	2,697,297	2,594,882
Total	236,024,438	246,683,748

Income Assessed in England and Wales for Income Tax Purposes.

——	Year 1919-20	Year 1920-21
	£	£ (Estimated)
Gross Income brought under review .	2,566,878,147	2,590,000,000
Deductions for exemptions, repairs to property, wear and tear, etc. . . .	350,183,094	415,000,000
Actual Income liable to tax before deduction of personal allowances, etc. . .	2,216,695,053	2,175,000,000

APPENDIX III.

* Income from the Ownership of Lands and Houses. Gross Assessments—Schedule A. England and Wales.

Year	Lands	Houses
	£	£
1814–15 . . .	37,063,000	14,895,000
1842–43 . . .	42,127,000	35,556,000
1850–1 . . .	42,790,000	39,354,000
1851–2 . . .	41,490,000	40,047,000
1857–8 . . .	42,895,000	47,439,000
1860–1 . . .	43,036,000	49,505,000
1861–2 . . .	44,686,000	53,235,000
1864–5 . . .	46,462,000	59,286,000
1867–8 . . .	47,767,000	68,013,000
1870–1 . . .	49,011,000	75,307,000
1876–7 . . .	52,016,000	90,451,000
1877–8 . . .	51,934,000	93,104,000
1879–80 . . .	52,041,000	100,079,000
1880–1 . . .	51,847,000	102,417,000
1882–3 . . .	48,659,000	109,374,000
1884–5 . . .	47,864,000	112,791,000
1885–6 . . .	46,255,000	115,436,000
1886–7 . . .	45,635,000	117,183,000
1887–8 . . .	44,732,000	118,524,000
1888–9 . . .	42,534,000	120,514,000
1890–1 . . .	41,635,000	123,721,000
1893–4 . . .	40,335,000	131,860,000
1894–5 . . .	39,942,000	133,512,000
1897–8 . . .	38,378,000	142,128,000
1898–9 . . .	37,526,000	149,632,000
1901–2 . . .	37,017,000	162,263,000
1904–5 . . .	36,896,000	177,666,000
1910–11 . . .	37,044,000	196,196,000
1911–12 . . .	36,990,000	197,632,000
1912–13 . . .	37,013,000	199,648,000
1913–14 . . .	37,071,000	202,018,000
1915–16 . . .	36,950,000	205,564,000
1917–18 . . .	36,910,000	207,495,000
1918–19 . . .	36,900,000	207,648,000

* Extracted from 'British Incomes and Property,' by Sir Josiah Stamp, K.B.E., D.Sc.

REPORTS ON THE STATE OF SCIENCE

ETC.

Seismological Investigations.—*Twenty-seventh Report of Committee* (Professor H. H. TURNER, *Chairman*; Mr. J. J. SHAW, *Secretary;* Mr. C. VERNON BOYS, Dr. J. E. CROMBIE, Sir HORACE DARWIN, Sir F. W. DYSON, Sir R. T. GLAZEBROOK, Dr. HAROLD JEFFREYS, Professors C. G. KNOTT *and* H. LAMB, Sir J. LARMOR, Dr. A. CRICHTON MITCHELL, Professors A. E. H. LOVE, H. M. MACDONALD, *and* H. C. PLUMMER, Mr. W. E. PLUMMER, Professor R. A. SAMPSON, Sir A. SCHUSTER, Sir NAPIER SHAW, Dr. G. T. WALKER). *Drawn up by the Chairman except where otherwise mentioned.*

General.

The Committee has again to deplore the loss of one of its most eminent members in Mr. G. W. Walker, who was Director of the Eskdalemuir Observatory from 1908 to 1912. He took a keen interest in the work of the Committee, and in its twenty-second Report (1917) put forward his startling suggestion of a considerable depth for earthquake origins, to which he recurred in a paper to the Royal Society shortly before his death (*Phil. Trans.* A **222**, 45-46). Recent work has provided evidence tending in the direction he indicated, as is mentioned later in this Report.

The clerical work at Oxford is still being carried on in the 'Students' Observatory,' since the tenant of the house purchased by Dr. Crombie's benefaction declares himself still unable to find other quarters. But the loss of the expected greater convenience has not been allowed to interfere with the progress of the work.

At the meeting of the International Union for Geodesy and Geophysics in Rome, May 2 to 10, a Section of Seismology was constituted, of which Professor Rothé of Strasbourg was elected Secretary and Professor Turner (Chairman of the Committee) President. Mr. J. J. Shaw (Secretary of the Committee) acted as English Secretary throughout the meeting.

The French expressed a strong desire that the Central Bureau should be located at Strasbourg under Professor Rothé. Accordingly the President made this proposal from the Chair, though he would for certain reasons have preferred Oxford. But the most important of these reasons was the desire to maintain the continuity of the work started in England by Milne, and carried forward since his death by other members of this Committee; and this desire was essentially satisfied when the President was requested to continue the issue of the Bulletins of the Committee, and further to make them official bulletins of the Section of Seismology: 10,000 francs a year being set aside out of the funds of the Union towards the cost of them. It was understood that since the year 1917 has already been started in the name of this Committee, the conversion should commence with the bulletins for 1918. This arrangement is accordingly submitted for the approval of this Committee, and of the British Association and Royal Society, which have hitherto borne the expenses of this work. It should be explained, however, that the sum voted (10,000 francs annually) will not meet the expenses of computing and printing the bulletins as at present. The first instalment of 10,000 francs just received represents 193*l*. in English money. The British Association and the Royal Society have recently contributed 100*l*. and 300*l*. per annum respectively, making more than double the contribution which the Section of Seismology found itself able to afford at present. It is possible that the Section may increase its contribution in the future, but at Rome there was a general desire to go forward for some years without increasing the financial demands on Government if possible. Hence not only will the 100*l*. a year (permanently put at the disposal of the Seismological Committee by the British Association from the Caird Fund) still be necessary, but an additional 100*l*. a year will only keep things going as they were: and there is still no provision for a Director of the work. The whole situation is, however, now

clearer, and it will be possible for the Committee at an early meeting to consider it fully.

Immediate anxiety has been avoided by the action of the Government Grant Board in renewing its contribution of 300*l*. for the present year at the meeting in March 1922.

Instrumental.

The Milne-Shaw seismograph in the basement of the Clarendon Laboratory at Oxford has worked well throughout the year. A most curious disturbance to which the trace was liable very occasionally, and of which for some time no explanation could be assigned, has now been identified as due to pressure on the floor above at a particular point, transmitted by a pillar between the two floors; but further experiments will be made.

Miniature copies of films, as suggested in the last report, have been received from Edinburgh, Helwan, and one or two other observatories, and found very useful. Attention is again called to them in the hope that other observatories will send such copies at their convenience.

During the year Milne-Shaw machines have been despatched to Toronto (2) and Victoria, B.C. (2). In addition the opportunity of the eclipse expedition to Christmas Island was taken, with the approval of the Astronomer Royal, to send a Milne-Shaw equipment in the care of the eclipse observers, who will be on the island for some months. They were to set up the instrument as soon as the immediate requirements of the eclipse preparations were sufficiently satisfied, and to take records for the duration of their stay on the island. If a competent person could be found to take charge of the instrument after their departure it was to be left in his care; if not, to be brought home again. The outcome of this experiment is awaited with considerable interest, for the neighbourhood is an important one.

Bulletins and Tables.

The bulletins for January-February and for March-April, 1917, have been published. May-June was sent to the printer in May of last year, but owing to a shortage of suitable type (now remedied) it was ultimately decided to print May alone. It is now being printed off, and will shortly be distributed. June (1917) has been received in proof; July and August are ready for the printer, September and October nearly ready. Two reasons have contributed to this further delay (for in order to catch up arrears we must print in one year not less than twelve months but more). The first, the continued dropping in of new material—*e.g.* results for Vieques and Cheltenham in 1917 were only received a few days ago. It is true they were themselves in printed form, and printing has suffered in many ways lately; but the opportunity may be taken to repeat the request to observatories to send results *in MS.* as soon as possible, so that they may be collated with others. Further, it may be remarked that when copies of results are manifolded sometimes almost illegible examples are received. It would be a real kindness, and facilitate work, if poor copies could be omitted or doubtful figures rectified by hand.

The second reason originates in the gradual development of the work by the inclusion of smaller earthquakes. It is naturally these which give most trouble in identification, but it was thought desirable to make trial whether their inclusion could be managed with our present facilities, and it has been concluded from experience that the effort should be made. It will suffice to refer to the paragraph on Periodicity as evidence of the value of these smaller earthquakes.

Depth of Focus.

On March 3, 1922, the subject chosen for the Geophysical meeting of the Royal Astronomical Society was the depth of earthquake foci, with special reference to Mr. G. W. Walker's suggestion of a considerable depth—comparable with 0.2 of the earth's radius. The contribution made from this Committee was based on the study of the arrival of the first waves at the Antipodes, for which the standard formula

$$20\text{m. } 17\text{s.} - (180-\Delta)^2 \times 0^{\text{s}}{\cdot}0235$$

was found to give good results, with the reservation that any particular earthquake is liable to show a systematic deviation from the formula at all the antipodal stations. (Δ is the distance in degrees from the end of the diameter through the epicentre. The name 'hypocentre' was used for this point; but Mr. Davison calls attention to the fact that this name has been used, especially in Italy, for the focus; and it may therefore be better to use 'anticentre' for the antipodal point.) It was suggested that this systematic deviation is due to variation in depth of the focus; and this suggestion was reduced to numerical form by the help of the calculations made by Professor C. G. Knott. It would thus appear that, if we call d the unknown depth of the focus corresponding to the adopted tables, the depth for different earthquakes in the years 1913 to 1916 varied from $d-.021$ to $d+.061$, the earth's radius being unity. Hence $d-.021$.

The investigation has been printed as No. 1, Vol. I, of a series of Geophysical Supplements to the Monthly Notices of the Royal Astronomical Society, to which further reference may be made.

Another contribution to the discussion was made by Dr. Dorothy Wrinch, of which she has kindly supplied the following summary:—

In a problem such as that of the depth of earthquake foci, in which there is a tremendous variety of relevant data, it is important to select for first consideration those data which are the most fundamental. The data with respect to the speeds of P and S waves near the surface of the earth appear to be of prime importance in the discussion of this problem.

1. The records of the Oppau explosion last year at Strasbourg, Zurich, Munich, and De Bilt give a value of 5.4 km./sec. for the speed of the P waves.[1] (The data in question were obtained by the kind assistance of the Director of the Meteorological Office.)

2. From the density and elastic constants deduced from the observations of Adams and Coker and others a speed of 5.38 km./sec. was calculated for the P waves and a speed of 2.99 km./sec. for the S waves near the surface of the earth.

Assuming these results, we may obtain an upper limit for the depth of the focus of an earthquake from P or S observations. It is well known that the speed of P and S waves increases when their paths are no longer restricted to the surface layer of the earth. An upper limit to the focal depth of any single earthquake can therefore evidently be obtained from the observations of that earthquake, if we find the depth at which the focus would lie if we use these observations and assume that the waves are propagated throughout their path with the velocity which they have in the surface layer. Making this assumption, we can deduce that if d is the depth of the focus, R the radius of the earth, v the velocity of the wave in question (P or S), the time t to a distance Δ is given by

$$vt=\sqrt{\Delta^2+d^2}\left[1-\frac{1}{2}\frac{d}{\mathrm{R}}\cdot\frac{\Delta^2}{\Delta^2+d^2}\right],$$

if terms of relative order $(\Delta/\mathrm{R})^2$ are neglected. Now, in the case of earthquake records taken at stations near enough to the disturbance for the terms of order $\left(\frac{\Delta}{\mathrm{R}}\right)^2$ to be of no significance, when the probable experimental error of the records is taken into account, we obtain an upper limit for the depth of the focus of the earthquake by applying the formula above to the observations and inserting the values of v for P or S already agreed upon. This procedure in the case of the observations of the Calabrian earthquake of 1905, September 8, taken at stations at distances of between 40 and 220 km., yields 40-50 km. as an upper estimate of the depth. The same method applied to the observations of stations in the neighbourhood of the two Formosa earthquakes of 1906, April 14, yields a depth of order of 450 km. as an upper limit. (These investigations will be published in full shortly.)

With this method of obtaining information about an earthquake focus only the observations at different stations of one disturbance are used in any one

[1] Cf. a forthcoming paper in collaboration with Dr. H. Jeffreys on this problem.

deduction, instead of the observations of one or more stations of different disturbances. This characteristic of the method offers one important advantage. For when we attempt to obtain information as to the depth of earthquake foci by using observations of various disturbances the number of unknowns (viz. the actual depths) is equal to the number of different disturbances recorded in the observations. When the observations are all associated with a single disturbance only one focal depth is involved.

Depth of Focus for 1920, December 16.

As an example of the first method a note may be given on the great Chinese earthquake, of which some particulars were furnished in the last report. Adopting the epicentre 35°.5 N. 105°.5 E. and T_0=1920 Dec. 16d. 12h. 5m. 46s., as given in the last report, the records from twenty-seven stations now available give a mean correction to T_0 of only -0.5 sec. The mean numerical residual is ± 7.1 sec.; so that the probable error of the mean of 27 is about 1 sec., and we may regard T_0 as exceptionally well determined. If only we had as many stations near the anticentre we could determine the focal depth with great accuracy. But up to the present La Paz is the only station from which results have been received. The time at $\Delta = 160°.2$ was 20m. 12s. after T_0, or +4s. in *excess* of the adopted formula. So far as this evidence goes, then, the focus was slightly *above* the normal depth d; but news from other stations in South America would be of the greatest interest.

Earthquake Periodicity.

Hitherto the periods considered in connection with earthquakes have been long; recently attention has been directed to some periods near 300 years, and even the fifteen months on which much work has been done is very long compared with that now to be mentioned, which is of only twenty-one minutes. But the results obtained are so startling, and yet so well supported, that, although they are only a few weeks old, it seems desirable to give some account of them.

It is mentioned in a preceding paragraph that the work of collation has recently been extended to the smaller earthquakes, which are sometimes only scantily recorded and therefore difficult to identify. It is a great help that they often come from the same neighbourhood as a previous shock, and possibly, in many cases, from the same actual focus. At any rate, it is a convenience to refer them to the same focus, when discrepancies, if any, will be shown by the residuals.

On August 3 to 10, 1917, some twenty shocks were recorded by Mizusawa and Osaka, which clearly came from the same focus, or nearly the same. It was natural to inquire whether there was any regularity in the intervals between shocks, and (without any special anticipation of finding it) a period of 21m. presented itself. The half-period of 10.5m. and the third of 7m. are both liable to present themselves in one connection or another, but there seems little doubt that 21m. is the master-period.

In a preceding paragraph it is mentioned that 21m. is approximately the time taken by a P wave to go right through the earth. In the investigation there mentioned 20m. 17s. appears; but this is the time from a focus of the average depth to the opposite face, and must be shorter than the complete time from face to face. The defect from 21m. *may* therefore, if the interpretation suggested is correct, give us incidentally the depth of the average focus. But it is too early to consider this as more than a possibility.

It follows that 21m. is also the time taken by a P wave to travel to the earth's centre and back again to the surface, *i.e.* it is the time for a possible pulsation. It is again too early to put forward this view of the following facts definitively, but it may help to co-ordinate the facts if this possibility is kept in mind.

The following figures will show how the 21m. period appeared and what part was played by the half-period 10.5m. The first shock came at August 8d. 5h. 25.0m., and was followed by two others 2×21m. and 8×21m. later. The actual intervals were not 43m. and 168m., but 45.9m. and 170.6m., so that the shocks do not come precisely but only approximately at the expected

moments. (The period of 21m. itself is, however, obtained in what follows with remarkable precision to be 21.00155m.)

The next shock, however, did not come at a whole multiple, but nearer the half-multiple, and seemed to start a new series which continued for eight more shocks, when a third series nearer the original series began to return, and, after a short overlap with the second series, took sole command.

The three series may be given in tabular form showing simply the differences from an exact sequence of 21m., starting with the original shock as time-zero :—

Series I. and III.				Series II.			
No.	Multiple	Diff.	Resid.	No.	Multiple	Diff.	Resid.
		m.	m.			m.	m.
I.	0	0·0	−2·2	IV.	35	+12·4	−3·3
II.	2	3·9	+1·7	V.	40	+15·7	0·0
III.	3	2·6	+0·4	VI.	59	+19·2	+3·5
				VII.	60	+15·5	−0·2
	Mean	2·2	±1·2	VIII.	63	+11·2	−4·5
				IX.	82	+12·1	−3·6
XIII.	125	6·7	−0·5	X.	92	+17·3	+1·6
XV.	154	6·7	−0·5	XI.	105	+14·5	−1·2
XVIII.	174	4·6	−2·6	XII.	106	+15·2	−0·5
XIX.	179	10·0	+2·8				
XX.	181	10·9	+3·7	XIV.	153	+19·7	+4·0
XXI.	182	4·1	−3·1	XVI.	155	+18·7	+3·0
				XVII.	160	+17·0	+1·3
	Mean	7·2	±2·2		Mean	+15·7	±2·2

The strongest part of the evidence is the sequence of nine shocks from IV. to XII., after the first series has ended and before the third has begun.

If we take these by themselves the mean value is +14.8 and the mean of the errors falls to ± 2.0m. When we have overlapping series there is the objection that we could always choose two points in a cycle of 21m. which would reduce the range of the residuals from ±10.5m., with mean of the errors ± 5.2, to ± 5.2m. with mean of the errors ± 2.6m., which is not much greater than those found. But this objection loses its force when we have a consecutive series of nine shocks without departure from the same reference point.

Let us now consider the following two series of shocks from apparently the same epicentre (6°.0 S. 136°.0 E.), one set in July 1917 and one in August 1917.

Date				Multiple	Error	Date				Multiple	Error
	d.	h.	m.		m.		d.	h.	m.		m.
July	27	23	36·2	0	−2·9	Aug.	7	15	54·1	0	+1·4
	29	21	52·1	132	+1·0		9	23	6·8	158	−3·9
	30	2	4·0	144	+0·9		10	17	25·5	210	+2·8
	30	4	21·0	150	+1·4*		14	8	9·0	458	−1·7
	30	10	28·0	168	+0·9		21	15	22·5	958	+1·3*
	30	10	36·2	168	−1·4*		30	3	24·2	1541	0·0*
	30	13	49·8	177	+3·2*						
	30	16	22·5	185	−1·6						
	30	16	55·8	186	+0·2*						
	31	3	13·2	216	−1·9						
				Mean	±1·5					Mean	±1·8

In the six cases marked with an asterisk a half-period of 10.5m. has been subtracted from the actual error. The procedure is tolerably clear. In each set the starting point is chosen so as to make the mean of the errors zero, and can be recovered from the top case by reversing the error. (The minutes for July 27 are thus 36.2+2.9=39.1, and for August 7, 54.1−1.4=52.7.)

The accident of preparing the bulletins month by month led to the treatment of these two series separately. But we can easily test whether they join together

by dividing the interval between the last (standard) date in July and the first in August by 21m. We thus have :—

	d.	h.	m.	
Aug.	7	15	52·7	
July	31	3	15·1	Diff. $516 \times 21\text{m} + 1{\cdot}6$

The small difference of +1.6m. indicates a slight correction to the period. Spreading it over the interval between the means of the groups, which is about 870 periods, the correction indicated is about 1.6/870=.00184m., comparable with what we shall find below.

The Periodicity not Local.

The question thus arose whether this periodicity was confined to earthquakes from the same focus or was wider in scope. The July series above is of a few days only and might conceivably be a local phenomenon (in space and time), but the August series is scarcely in keeping with this view. If the earth generally is affected, we must find the space relationship at any particular moment. A few trials of shocks at about the same time in different localities suggested that the longitude is unimportant and that the effect spreads from the equator outwards to the poles. The rate of travel suggested that the journey from equator to either pole is completed in 21 minutes, but the first approximation was found to require sensible correction, the rate being certainly slower near the equator. Ultimately the following (quite provisional) table was found to give good results :—

Times for Different Latitudes.

Lat.	Time	Lat.	Time	Lat.	Time	Lat.	Time
	m.		m.		m.		m.
0°	0·0	20°	6·8	40°	15·5	60°	20·0
5	0·8	25	9·0	45	17·4	65	20·3
10	2·5	30	11·1	50	18·7	70	20·6
15	4·6	35	13·3	55	19·4	75	20·8

The information about very high latitudes is scanty, and 21m. may be set down for the poles without much fear of contradiction at present.

When all the earthquakes of 1917 were discussed in this way a distinct periodicity in 21m. was manifest, but with some curious features requiring further examination. After a good deal of work, and the use of all the large earthquakes from 1913-16, the following features of the earth movement emerged :—

(*a*) There is a distinct oscillation of the maximum in six months, the solstices and equinoxes being the epochs of extreme range, which is about 120°, or a one-third-period of 7 min.

(*b*) There is no sensible annual oscillation, but

(*c*) There is another oscillation, about 180° in amplitude (or 10.5 min.), in twenty-four months, with spring equinoxes as extreme epochs. To so unexpected a feature it is difficult at present to assign a meaning, but the very difficulty lends some support to its reality. To show the kind of evidence for it the following figures may be given. After removal of the six-month periodicity (*a*), the results were collected in groups of six months (which would render any still uncorrected six-monthly term insensible), and the maximum computed by harmonic analysis. The phases of the maxima came out as follows for the nine sets (of six months each) available, viz. two each in the years 1913, 1914, 1915, and 1916, and one in 1917 which is not yet fully reduced. They are placed in sets of four to show the 24-monthly term :—

Years	Maximum Phases			
	°	°	°	°
1913 and 1914 .	113	175	280	160
1915 and 1916 .	135	202	345	185
1917 . . .	143			

These figures show a fluctuation, with a slow increase. Comparing corresponding pairs at two years' interval we get for the increases :—

$$+22°, +27°, +65°, +25°, +8° : \text{ mean } +29°$$

Considering the nature of the material, these are wonderfully consistent. Applying the appropriate correction (which represents, of course, error in adopted period) we get :—

°	°	°	°
113	168	266	139
107	167	303	136
87			

The range is thus about 180° ; a more exact value cannot be assigned at present. The first column corresponds to March 1913, 1915, &c., and the third to March 1914, 1916, &c.

The correction to period is about 29° per two years, which contain some 48,000 periods, and thus represents 0.000036m. per period. The value previously adopted being 21.001512, we have as the corrected period 21.001548m.

Using this period and allowing for the 6- and 24-monthly fluctuations of maximum already mentioned, and using the table for latitude above given, the numbers of earthquakes in each minute of the twenty-one for the years 1913-16 (taken together) and for 1917 January-September (incomplete, but with many more determinations of epicentres) are as follows. The starting point has been chosen so that the maximum falls in the middle of the series :—

Minute	1913-16	1917	Minute	1913-16	1917	Minute	1913-16	1917
1	11	14	8	18	13	15	12	15
2	13	10	9	22	13	16	13	6
3	10	8	10	15	13	17	12	6
4	10	7	11	19	11	18	10	8
5	13	6	12	10	9	19	10	7
6	10	13	13	13	12	20	13	6
7	12	12	14	17	10	21	15	9
Sum	79	70	Sum	114	81	Sum	85	57

The middle column is thus sensibly, though not extravagantly, greater than the wings. But the special features of the variation are made clearer by arranging the series in two halves which are symmetrical with respect to the maximum. The number of minutes being odd, one item is left without a partner, and may be doubled as a rough expedient :—

Minutes . .	11 & 12	10 & 13	9 & 14	8 & 15	7 & 16	6 & 17	5 & 18	4 & 19	3 & 20	2 & 21	1
1913-16 . .	19	15	22	18	12	10	13	10	10	13	11
	10	13	17	12	13	12	10	10	13	15	
Sum .	29	28	39	30	25	22	23	20	23	28	(22)
1917 . . .	11	13	13	13	12	13	6	7	8	10	14
	9	12	10	15	6	6	8	7	6	9	
Sum .	20	25	23	28	18	19	14	14	14	19	(28)
Total .	49	53	62	58	43	41	37	34	37	47	(50)

The special feature is clearest in the line of totals, but can be recognised in each of the others. The position assigned to the maximum by the harmonic of the first order is on the left, *between* the minutes 11 and 12 ; but the largest figures occur symmetrically on either side of this point between minutes 9 and 8 or 14 and 15, *i.e.* about $3\frac{1}{2}$m. away from the mid-point, the total separation

being about 7m., or one-third of the 21. It seems probable that this feature is related to the liability mentioned earlier but not yet illustrated. An example may now be given. In Seismological Notes No. 2 (dated March 1922 and received in the early stages of this investigation), Prof. Omori deals with 'The Severe Earthquake of December 8, 1921,' and his assistant, Mr. Yasida, gives a list of forty-four after-shocks. If these are arranged in lengths of 21m., taking 21h. 31.7m., the time of the main shock, as origin, then the counts for consecutive minutes are as below :—

Minute	Count	Minute	Count	Minute	Count	Sum of Count
1	0	8	1	15	0	1
2	2	9	1	16	1	4
3	4	10	1	17	2	7
4	3	11	3	18	1	7
5	5	12	2	19	6	13
6	3	13	0	20	0	3
7	0	14	1	21	2	3

The sum of the three columns of counts given in the last column shows an obvious maximum and minimum. This was the way in which the matter presented itself at the time—there is a noticeable third harmonic. But in the light of the remarks just made we should now arrange the material symmetrically on both sides of the main shock (which is the zero-point) as below :—

Minute . .	1 & 21	2 & 20	3 & 19	4 & 18	5 & 17	6 & 16	7 & 15	8 & 14	9 & 13	10 & 12	11
Counts . .	0	2	4	3	5	3	0	1	1	1	3
	2	0	6	1	2	1	0	1	0	2	
Sum . .	2	2	10	4	7	4	0	2	1	3	(6)

Comparing this line with the 'Total' line of 1913-17, we can hardly doubt that the feature, which is a plain fact of observation with regard to a simple series of repeated shocks, has been traced in the whole mass of earthquakes wherever they occur. It is not so emphatic as in the simple Tokyo case—that is scarcely to be expected, for several systematic terms have only been roughly evaluated, especially the correction for latitude—but it is clearly there; and we can scarcely doubt that the earth generally is subject to some disturbance of an oscillatory character with this period of 21.001548m., which has been traced already over nearly five years and may possibly be permanent. The matter must be left at this point, but naturally investigations are being continued.

Tides.—*Report of Committee to assist work on the Tides* (Professor H. LAMB, *Chairman*; Dr. A. T. DOODSON, *Secretary*; Col. Sir C. F. CLOSE, Dr. P. H. COWELL, Sir H. DARWIN, Dr. G. H. FOWLER, Admiral F. C. LEARMONTH, Sir J. E. PETAVEL, Professor J. PROUDMAN, Major G. I. TAYLOR, Professor D'ARCY W. THOMPSON, Sir J. J. THOMSON, Professor H. H. TURNER).

Investigations carried out during the year, and still under consideration, at the Tidal Institute at Liverpool have been concerned with

1. Attempts to reduce to law the residual semi-diurnal oscillations left from the tide-gauge record at Newlyn;
2. Meteorological effects at Newlyn, Liverpool, and other places;

3. Long-period tides at Newlyn;
4. The perturbations of harmonic constants.

Considerable progress has been made, but as the problems are all connected together it is thought desirable to postpone publication. In the case of (4) the 'constants' of harmonic analyses over many years for tides in Indian and Canadian waters show variations of period of nineteen years, indicating that the assumed theoretical variation of harmonic constituents in the period of revolution of the moon's nodes is not sufficiently accurate. The results so far show the presence of a harmonic term not present in the astronomical forces, agreeing with previous deductions from Newlyn as to the possibility of such new constituents. Hypotheses concerning the physical nature of these variations are being tested.

Investigation of the Upper Atmosphere.—*Report of Committee* (Sir Napier Shaw, *Chairman*; Mr. C. J. P. Cave, *Secretary*; Professor S. Chapman, Mr. J. S. Dines, Mr. W. H. Dines, Sir R. T. Glazebrook, Colonel E. Gold, Dr. H. Jeffreys, Sir J. Larmor, Mr. R. G. K. Lempfert, Professor F. A. Lindemann, Dr. W. Makower, Sir J. E. Petavel, Sir A. Schuster, Dr. G. C. Simpson, Mr. F. J. W. Whipple, Professor H. H. Turner).

The Committee met in December 1921. A memorandum by the Chairman and Secretary was read leading up to four propositions as follows :—

(1) That a representation of the desirability of inviting the co-operation and inter-co-operation, not only of the Directors of Institutes and Observatories, but also of Scientific Academies and Societies in the study of the upper air, should be put forward to the British National Committee with a view to the meeting of the International Union of Geodesy and Geophysics at Rome in April 1922.

(2) That the attention of the authorities in charge of regular observations of the upper air should be drawn to the close correlation between variations of temperature in the middle layers of the atmosphere derived from Mr. W. H. Dines's observations and from Canadian observations, and that an examination of the observations in other localities from the same standpoint should be undertaken.

(3) That endeavour should be made to provide suitable instructions for observations with pilot-balloons of long carry both on sea and on land.

(4) That endeavour should be made to enlist the co-operation of competent persons willing to carry out observations in localities of special interest, and particularly to obtain the co-operation of astronomical observatories on sites specially free from cloud.

In respect of proposition (1) a memorandum for communication to the National Committee was approved.

In respect of proposition (2) it was agreed that the subject should be raised and the suggestion incorporated in the report to the British Association.

In respect of (3), that the question be referred to the Secretary and Captain D. Brunt, acting for the Director of the Meteorological Office, in order that definite proposals may be formulated.

In respect of (4) it was suggested that communication should be held with Captain H. Douglas, formerly Head of the Naval Meteorological Service, Assistant Hydrographer, and now commanding a surveying ship at Bermuda, as to whether it is possible to initiate pilot-balloon observations at Bermuda. Dr. Simpson undertook to conduct the communication, as the Meteorological Office was in a position to supply gear if necessary.

The subjects suggested in propositions (1), (3), and (4) were incorporated in the programme put forward for consideration at Rome by the British Committee.

The Committee has been informed that the subjects were approved by the Union, and that the Section for Meteorology was authorised to spend a sum of 15,000 francs upon providing instruments for duly qualified observers by *ballons*

sondes from on board ship or from aeroplane, and the Bureau of the Section was instructed to make an appeal to Yacht Clubs and Aero Clubs for assistance in obtaining observations from localities where otherwise no observations would be available. Further sums are at the disposal of the Section for the same purpose from the revenue of future years if progress with the inquiry in this direction is found to be practicable.

The Committee has been further informed that the Section for Meteorology has instructed its Bureau to endeavour to secure observations of the direction and velocity of the wind in the stratosphere by means of pilot-balloons, and for this purpose to appeal to the directors of astronomical or other observatories which have been placed in exceptionally favourable situations for clear atmosphere. Such observatories are generally located in the belt of high pressure for which observations of the stratosphere are specially desired. The Executive Committee of the Section has been authorised to devote a sum of 5,000 francs, and a like amount in the three succeeding years if necessary, in order to provide the instruments for making these observations, to be lent to the observatories concerned.

With regard to Resolution (2), it may be recalled that in the 'Characteristics of the Free Atmosphere' (*Geophysical Memoirs*, vol. 2, p. 67), Mr. W. H. Dines gave a table of coefficients of correlation between pressure and temperature over England. The observations were dealt with in three monthly groups. The figures for Canada, obtained by Mr. J. Patterson with apparatus supplied by Mr. Dines, and forming a homogeneous series, have been similarly treated.[1] About sixty altogether, they are not numerous enough to group in separate quarters of the year. They show very high correlation for the year, which implies high temperature and high pressure in the summer and low pressure and low temperature in the winter. The dot diagrams show, however, that the relation extends also to the individual quarters. And in any case the extension of the cold in winter to eight kilometres in association with low pressure is a very striking fact.

Dr. van Bemmelen's observations at Batavia have also been examined from this point of view; and the Chief of the U.S. Weather Bureau has been asked for the individual values of the ascents in U.S.A. for the same purpose.

The results are shown in the following table :—

Table of Coefficients of Correlation Between Variations of Pressure and Temperature in the Upper Air of England and of Canada.

Height in km.	England				Canada	Batavia
	Jan.—Mar.	Apl.—June	July—Sept.	Oct.—Dec.	Year	Year
14	—	—	—	—	—	·76
12	—·38	—·24	—·41	—·34	·04	·86
10	·35	·20	·43	·29	·78	·88
8	·91	·81	·87	·86	·93	·72
6	·84	·92	·83	·85	·93	·64
4	·86	·89	·75	·83	·88	·38
2	·82	·49	·56	·76	—	·02
0	—·02	·14	—·02	·33	nil	—

With regard to proposition (4), it is understood that the arrangements for observations with pilot-balloons at Bermuda are going forward.

The Committee think that the Association may usefully keep the subject in mind, and therefore ask for reappointment.

[1] Shaw, *The Air and its Ways.* C.U. Press, p. 101.

Calculation of Mathematical Tables.—*Report of Committee* (Professor J. W. NICHOLSON, *Chairman*; Dr. J. R. AIREY, *Secretary*; Mr. T. W. CHAUNDY, Professor L. N. G. FILON, Colonel HIPPISLEY, Professor E. W. HOBSON, Mr. G. KENNEDY, *and* Professors ALFRED LODGE, A. E. H. LOVE, H. M. MACDONALD, G. N. WATSON, *and* A. G. WEBSTER).

Since the last report of the Committee in 1919, a number of tables of functions have been computed, including Bessel-Clifford and Lommel-Weber functions of zero and unit orders and Lommel-Weber and other related functions of equal order and argument.

Dr. Doodson contributes in Part I of this report a set of tables of the Riccati-Bessel functions in continuation of the tables already published in the 1914 and 1916 reports, where they are incorrectly described as Bessel functions of half integral order.

Part II contains a table of the zeros of Bessel functions of high order to which reference was made in the 1916 Report.

It is proposed to defer the publication of the following tables which have been calculated without the assistance of a grant from the Committee:—

Sin θ and Cos θ to fifteen places of decimals for θ in circular measure from 1 to 100 radians. These were originally computed to twenty-four places of decimals.

Bessel-Clifford functions, $C_0(x)$ and $C_1(x)$ to six places of decimals for $x=0{\cdot}00$ to $20{\cdot}00$ by intervals of $0{\cdot}02$.

Lommel-Weber functions, $\Omega_0(x)$ and $\Omega_1(x)$ to six places of decimals for $x=0{\cdot}00$ to $16{\cdot}00$ by intervals of $0{\cdot}02$.

Bessel, Neumann, Schläfli and Lommel-Weber functions to six places of decimals where the order and argument are equal or differ by unity, the values of the order ranging from 0 to 10 by intervals of 0·25.

PART I.

Riccati-Bessel Functions.

($x = 0{\cdot}1$ to $x = 0{\cdot}9$.)

These functions are defined in the Reports of the British Association for the Advancement of Science, 1914 and 1916, which contain tables for $x=1, 2 \ldots 10$, and $x=1{\cdot}1, 1{\cdot}2, \ldots \ldots 1{\cdot}9$, respectively.

NOTE ON A METHOD OF INTERPOLATION.

The Riccati-Bessel functions are subject to the following relations:—

$$E_n(x)=C_n(x)-\sqrt{-1}.S_n(x)$$

$$E_n'(x)=\frac{n+1}{x}\,.\,E_n(x)-E_{n+1}(x)$$

$$E_n'(x)=E_{n-1}(x)-\frac{n}{x}\cdot E_n(x)$$

$$E_{n+1}(x)=\frac{2n+1}{x}\cdot E_n(x)-E_{n-1}(x)$$

From the first and second equations we may obtain respectively:—

$$E_n(X)=\frac{X^{n+1}}{x^{n+1}}\left\{E_n(x)-\left(\frac{u}{2x}\right)E_{n+1}(x)+\frac{1}{2!}\left(\frac{u}{2x}\right)^2.E_{n+2}(x)-\ldots\ldots\right\} \quad \ldots \text{ I.}$$

$$E_n(X)=\frac{x^n}{X^n}\left\{E_n(x)+\left(\frac{u}{2x}\right).E_{n-1}(x)+\frac{1}{2!}\left(\frac{u}{2x}\right)^2.E_{n-2}(x)+\ldots\ldots\right\} \quad \ldots \text{ II.}$$

where $X^2=x^2+u^2$.

The Series of Type I for $S_n(x)$ and the Series of Type II for $C_n(x)$ are absolutely convergent for all values of u, n, x.

The Series of Type I for $C_n(x)$ is absolutely convergent for all values of n if $x^2 - u > 0$ and the Series of Type II for $S_n(x)$. for all values of n if $x^2 + u > 0$.

These series are of great use for interpolation purposes and are far more serviceable than the usual expansion by Taylor's Series: the latter involves the calculation of the derivatives and this is a troublesome matter as a rule. Within the range $0 < n < x$, it is best to use Type I for $S_n(x)$ and Type II for $C_n(x)$.

Riccati-Bessel Functions.

$x = 0{\cdot}1, 0{\cdot}2, \ldots\ldots, 0{\cdot}9.$

n	$S_n(0{\cdot}1)$	$C_n(0{\cdot}1)$	$\lvert E_n(0{\cdot}1)\rvert^2$	n
0	·0998334	0·9950042	1·0	0
1	·0033300	10·049875	101·0	1
2	·0000666	300·5012		
3	·0000010	15015·013		

n	$S_n'(0{\cdot}1)$	$C_n'(0{\cdot}1)$	$\lvert E_n'(0{\cdot}1)\rvert^2$	n
0	·9950042	—·0998334	1·0	0
1	·0665334	—99·50375	9901·0	1
2	·0019976	—5999·975		
3	·0000381	—450149·9		

n	$\log\lvert S_n(0{\cdot}1)\rvert$	$\log\lvert C_n(0{\cdot}1)\rvert$	$\log\lvert E_n(0{\cdot}1)\rvert^2$	n
0	$\bar{2}$·9992759	$\bar{1}$·9978249	0·0000000	0
1	$\bar{3}$·5224444	1·0021607	2·0043214	1
2	$\bar{5}$·8235985	2·4778463		
3	$\bar{7}$·9785694	4·1765257		

n	$\log\lvert S_n'(0{\cdot}1)\rvert$	$\log\lvert C_n'(0{\cdot}1)\rvert$	$\log\lvert E_n'(0{\cdot}1)\rvert^2$	n
0	$\bar{1}$·9978249	$\bar{2}$·9992759	0·0000000	0
1	$\bar{2}$·8230397	1·9978394	3·9956791	1
2	$\bar{3}$·3005129	3·7781494		
3	$\bar{5}$·5805087	5·6533571		

n	$S_n(0{\cdot}2)$	$C_n(0{\cdot}2)$	$\lvert E_n(0{\cdot}2)\rvert^2$	n
0	·1986693	0·9800666	1·0	0
1	·0132801	5·099002	26·0	1
2	·0005318	75·50497		
3	·0000152	1882·525		
4	·0000003	65812·88		

Riccati-Bessel Functions—contd.

n	$S_n'(0{\cdot}2)$	$C_n'(0{\cdot}2)$	$\lvert E_n'(0{\cdot}2)\rvert^2$	n
0	·9800666	—·1986693	1·0	0
1	·1322690	—24·51494	601·0	1
2	·0079620	—749·9507		
3	·0003037	—28162·37		
4	·0000084	—1314375·0		

n	$\log\lvert S_n(0{\cdot}2)\rvert$	$\log\lvert C_n(0{\cdot}2)\rvert$	$\log\lvert E_n(0{\cdot}2)\rvert^2$	n
0	$\bar{1}$·2981308	$\bar{1}$·9912556	0·0000000	0
1	$\bar{2}$·1232006	0·7074852	1·4149733	1
2	$\bar{4}$·7257575	1·8779755		
3	$\bar{5}$·1819654	3·2747408		
4	$\bar{7}$·5289284	4·8183109		

n	$\log\lvert S_n'(0{\cdot}2)\rvert$	$\log\lvert C_n'(0{\cdot}2)\rvert$	$\log\lvert E_n'(0{\cdot}2)\rvert^2$	n
0	$\bar{1}$·9912556	$\bar{1}$·2981308	0·0000000	0
1	$\bar{1}$·1214579	1·3894309	2·7788745	1
2	$\bar{3}$·9010202	2·8750327		
3	$\bar{4}$·4825124	4·4496692		
4	$\bar{6}$·9265524	6·1187193		

n	$S_n(0{\cdot}3)$	$C_n(0{\cdot}3)$	$\lvert E_n(0{\cdot}3)\rvert^2$	n
0	·2955202	0·9553365	1·0	0
1	·0297309	3·479975	12·11111	1
2	·0017885	33·84442		
3	·0000768	560·5936		
4	·0000026	13046·67		
5	·0000001	390839·6		

n	$S_n'(0{\cdot}3)$	$C_n'(0{\cdot}3)$	$\lvert E_n'(0{\cdot}3)\rvert^2$	n
0	·9553365	—·2955202	1·0	0
1	·1964173	—10·644581	113·34568	1
2	·0178078	—222·1495		
3	·0010209	—5572·092		
4	·0000426	—173395·0		
5	·0000014	—6500946·		

n	$\log\lvert S_n(0{\cdot}3)\rvert$	$\log\lvert C_n(0{\cdot}3)\rvert$	$\log\lvert E_n(0{\cdot}3)\rvert^2$	n
0	$\bar{1}$·4705872	$\bar{1}$·9801564	0·0000000	0
1	$\bar{2}$·4732076	0·5415761	1·0831840	1
2	$\bar{3}$·2524786	1·5294870		
3	$\bar{5}$·8851233	2·7486481		
4	$\bar{6}$·4083973	4·1154998		
5	$\bar{8}$·8443994	5·5919986		

Riccati-Bessel Functions—contd.

n	$\log\lvert S_n'(0{\cdot}3)\rvert$	$\log\lvert C_n'(0{\cdot}3)\rvert$	$\log\lvert E_n'(0{\cdot}3)\rvert^2$	n
0	$\bar{1}$·9801564	$\bar{1}$·4705872	0·0000000	0
1	$\bar{1}$·2931798	1·0271286	2·054405	1
2	$\bar{2}$·2506107	2·3466453		
3	$\bar{3}$·0089739	3·7460183		
4	$\bar{5}$·6295343	5·2390367		
5	$\bar{6}$·1449277	6·8129766		

n	$S_n(0.4)$	$C_n(0.4)$	$\lvert E_n(0.4)\rvert^2$	n
0	·3894183	0·9210610	1·0	0
1	·0524849	2·692071	7·25	1
2	·0042181	19·26947	371·3125	2
3	·0002417	238·1763		
4	·0000108	4148·816		
5	·0000004	93110·18		

n	$S_n'(0{\cdot}4)$	$C_n'(0{\cdot}4)$	$\lvert E_n'(0.4)\rvert^2$	n
0	·9210610	—·3894183	1·0	0
1	·2582062	—5·809116	33·8125	1
2	·0313943	—93·65528	8771·3125	2
3	·0024057	—1767·053		
4	·0001341	—41249·98		
5	·0000059	—1159728·5		

n	$\log\lvert S_n(0{\cdot}4)\rvert$	$\log\lvert C_n(0.4)\rvert$	$\log\lvert E_n(0{\cdot}4)\rvert^2$	n
0	$\bar{1}$·5904164	$\bar{1}$·9642884	0·0000000	0
1	$\bar{2}$·7200341	0·4300865	0·8603380	1
2	$\bar{3}$·6251190	1·2848698	2·5697396	2
3	$\bar{4}$·3831872	2·3768986		
4	$\bar{5}$·0317079	3·6179242		
5	$\bar{7}$·5928618	4·9689972		

n	$\log\lvert S_n'(0{\cdot}4)\rvert$	$\log\lvert C_n'(0{\cdot}4)\rvert$	$\log\lvert E_n'(0.4)\rvert^2$	n
0	$\bar{1}$·9642884	$\bar{1}$·5904164	0·0000000	0
1	$\bar{1}$·4119666	0·7641101	1·5290773	1
2	$\bar{2}$·4968502	1·9715323	3·9430646	2
3	$\bar{3}$·3812495	3·2472495		
4	$\bar{4}$·1273512	4·6154238		
5	$\bar{6}$·7680605	6·0643563		

Riccati-Bessel Functions—contd.

n	$S_n(0\cdot5)$	$C_n(0.5)$	$\lvert E_n(0\cdot5)\rvert^2$	n
0	·4794255	0·8775826	1·0	0
1	·0812685	2·234591	5·0	1
2	·0081856	12·529961	157·0	2
3	·0005870	123·06502		
4	·0000327	1710·3804		
5	·0000015	30663·78		
6	·0000001	672892·8		

n	$S_n'(0\cdot5)$	$C_n'(0\cdot5)$	$\lvert E_n'(0\cdot5)\rvert^2$	n
0	·8775826	—·4794255	1·0	0
1	·3168885	—3·591599	13·0	1
2	·0485263	—47·88525	2293·	2
3	·0046634	—725·8602		
4	·0003255	—13559·978		
5	·0000178	—304927·4		
6	·0000008	—8044050·		

n	$\log\lvert S_n(0\cdot5)\rvert$	$\log\lvert C_n(0\cdot5)\rvert$	$\log\lvert E_n(0\cdot5)\rvert^2$	n
0	$\bar{1}$·6807212	$\bar{1}$·9432880	0·0000000	0
1	$\bar{2}$·9099223	0·3491980	0·6989700	1
2	$\bar{3}$·9130480	1·0979497	2·1958997	2
3	$\bar{4}$·7686512	2·0901346		
4	$\bar{5}$·5144787	3·2330927		
5	$\bar{6}$·1728169	4·4866257		
6	$\bar{8}$·7584013	5·8279459		

n	$\log\lvert S_n'(0\cdot5)\rvert$	$\log\lvert C_n'(0\cdot5)\rvert$	$\log\lvert E_n'(0\cdot5)\rvert^2$	n
0	$\bar{1}$·9432880	$\bar{1}$·6807212	0·0000000	0
1	$\bar{1}$·5009065	0·5552878	1·1139434	1
2	$\bar{2}$·6859772	1·6802018	3·3604041	2
3	$\bar{3}$·6687070	2·8608530		
4	$\bar{4}$·5124967	4·1322590		
5	$\bar{5}$·2506022	5·4841965		
6	$\bar{7}$·9034930	6·9054748		

n	$S_n(0\cdot6)$	$C_n(0\cdot6)$	$\lvert E_n(0\cdot6)\rvert^2$	n
0	·5646425	0·8253356	1·0	0
1	·1157352	1·9402018	3·777778	1
2	·0140334	8·875674	78·77778	2
3	·0012098	72·02374		
4	·0000809	831·4013		
5	·0000044	12399·00		
6	·0000002	226483·5		

Riccati-Bessel Functions—contd.

n	$S_n'(0·6)$	$C_n'(0·6)$	$\|E_n'(0·6)\|^2$	n
0	·8253356	—·5646425	1·0	0
1	·3717505	—2·408334	5·938272	1
2	·0689572	—27·64538	764·2716	2
3	·0079844	—351·2430		
4	·0006701	—5470·652		
5	·0000441	—102493·57		
6	·0000024	—2252436·		

n	$\log\|S_n(0·6)\|$	$\log\|C_n(0.6)\|$	$\log\|E_n(0·6)\|^2$	n
0	$\bar{1}$·7517735	$\bar{1}$·9166306	0·0000000	0
1	$\bar{1}$·0634654	0·2878469	0·5772364	1
2	$\bar{2}$·1471628	0·9482013	1·8964037	2
3	$\bar{3}$·0827140	1·8574757		
4	$\bar{5}$·9082088	2·9198107		
5	$\bar{6}$·6460641	4·0933865		
6	$\bar{7}$·3110757	5·3550366		

n	$\log\|S_n'(0·6)\|$	$\log\|C_n'(0·6)\|$	$\log\|E_n'(0·6)\|^2$	n
0	$\bar{1}$·9166306	$\bar{1}$·7517735	0·0000000	0
1	$\bar{1}$·5702516	0·3817167	0·7736601	1
2	$\bar{2}$·8385795	1·4416225	2·8832477	2
3	$\bar{3}$·9022418	2·5456077		
4	$\bar{4}$·8261684	3·7380391		
5	$\bar{5}$·6440513	5·0106966		
6	$\bar{6}$·3765288	6·3526525		

n	$S_n(0·7)$	$C_n(0·7)$	$\|E_n(0·7)\|^2$	n
0	·6442177	0·7648422	1·0	0
1	·1554688	1·7368494	3·040816	1
2	·0220771	6·678798	44·60683	2
3	·0022251	45·96885	2113·135	3
4	·0001739	453·0097		
5	·0000111	5778·442		
6	·0000006	90351·07		

n	$S_n'(0·7)$	$C_n'(0·7)$	$\|E_n'(0·7)\|^2$	n
0	·7648422	—·6442177	1·0	0
1	·4221194	—1·7163712	3·124115	1
2	·0923912	—17·345431	300·8725	2
3	·0125410	—190·33056	36225·72	3
4	0012312	—2542·658		
5	·0000946	—40821·57		
6	·0000060	—768659·3		

Riccati-Bessel Functions—contd.

n	$\log\|S_n(0·7)\|$	$\log\|C_n(0.7)\|$	$\log\|E_n(0·7)\|^2$	n
0	$\bar{1}$·8090326	$\bar{1}$·8835718	0·0000000	0
1	$\bar{1}$·1916432	0·2397622	0·4829902	1
2	$\bar{2}$·3439429	0·8246983	1·6494014	2
3	$\bar{3}$·3473510	1·6624636	3·3249273	3
4	$\bar{4}$·2403688	2·6561075		
5	$\bar{5}$·0455687	3·7618107		
6	$\bar{7}$·7778181	4·9559333		

n	$\log\|S_n'(0·7)\|$	$\log\|C_n'(0.7)\|$	$\log\|E_n'(0·7)\|^2$	n
0	$\bar{1}$·8835718	$\bar{1}$·8090326	0·0000000	0
1	$\bar{1}$·6254353	0·2346112	0·4947270	1
2	$\bar{2}$·9656308	1·2391851	2·4783825	2
3	$\bar{2}$·0983312	2·2795085	4·5590171	3
4	$\bar{3}$·0903408	3·4052880		
5	$\bar{5}$·9758781	4·6108897		
6	$\bar{6}$·7757827	5·8857339		

n	$S_n(0·8)$	$C_n(0·8)$	$\|E_n(0·8)\|^2$	n
0	·7173561	0·6967067	1·0	0
1	·1999884	1·5882395	2·562500	1
2	·0326004	5·259191	27·66016	2
3	·0037643	31·28171	978·5452	3
4	·0003368	268·4557		
5	·0000246	2988·845		
6	·0000015	40828·17		
7	·0000001	660468·9		

n	$S_n'(0·8)$	$C_n'(0·8)$	$\|E_n'(0·8)\|^2$	n
0	·6967067	—·7173561	1·0	0
1	·4673706	—1·288593	1·8789063	1
2	·1184873	—11·55974	133·64160	2
3	·0184845	—112·0472	12554·577	3
4	·0020803	—1310·997		
5	·0001830	—18411·83		
6	·0000132	—303222·4		
7	·0000008	—5738275·		

n	$\log\|S_n(0·8)\|$	$\log\|C_n(0·8)\|$	$\log\|E_n(0·8)\|^2$	n
0	$\bar{1}$·8557348	$\bar{1}$·8430500	0·0000000	0
1	$\bar{1}$·3010048	0·2009160	0·4086639	1
2	$\bar{2}$·5132233	0·7209190	1·4418546	2
3	$\bar{3}$·5756788	1·4952904	2·9905809	3
4	$\bar{4}$·5273557	2·4288727		
5	$\bar{5}$·3910075	3·4755034		
6	$\bar{6}$·1815854	4·6109599		
7	$\bar{8}$·9096777	5·8198524		

Riccati-Bessel Functions—contd.

n	$\log\|S_n'(0{\cdot}8)\|$	$\log\|C_n'(0{\cdot}8)\|$	$\log\|E_n'(0{\cdot}8)\|^2$	n
0	$\bar{1}$·8430500	$\bar{1}$·8557348	0·0000000	0
1	$\bar{1}$·6696614	0·1101156	0·2739051	1
2	$\bar{1}$·0736720	1·0629480	2·1259417	2
3	$\bar{2}$·2668072	2·0494010	4·0988021	3
4	$\bar{3}$·3181294	3 1175994		
5	$\bar{4}$·2624788	4·2650969		
6	$\bar{5}$·1209316	5·4817613		
7	$\bar{7}$·9076251	6·7587813		

n	$S_n(0{\cdot}9)$	$C_n(0{\cdot}9)$	$\|E_n(0{\cdot}9)\|^2$	n
0	·7833269	0·6216100	1·0	0
1	·2487533	1·4740047	2·234568	1
2	·0458506	4·291739	18·421125	2
3	·0059725	22·36899	500·3717	3
4	·0006022	169·68929		
5	·0000496	1674·5239		
6	·0000034	20296·71		
7	·0000002	291500·2		

n	$S_n'(0{\cdot}9)$	$C_n'(0{\cdot}9)$	$\|E_n'(0{\cdot}9)\|^2$	n
0	·6216100	—·7833269	1·0	0
1	·5069344	—1·0161730	1·2895899	1
2	·1468630	—8·0631928	65·03665	2
3	·0259423	—70·27156	4938·093	3
4	·0032960	—731·8056		
5	·0003269	—9133·221		
6	·0000266	—133636·9		
7	·0000018	—2246927·		

n	$\log\|S_n(0{\cdot}9)\|$	$\log\|C_n(0{\cdot}9)\|$	$\log\|E_n(0{\cdot}9)\|^2$	n
0	$\bar{1}$·8939430	$\bar{1}$·7935180	0·0000000	0
1	$\bar{1}$·3957688	0·1684989	0·3491936	1
2	$\bar{2}$·6613454	0·6326333	1·2653161	2
3	$\bar{3}$·7761568	1·3496464	2·6992927	3
4	$\bar{4}$·7797452	2·2296544		
5	$\bar{5}$·6950724	3·2238914		
6	$\bar{6}$·5371851	4·3074257		
7	$\bar{7}$·3167215	5·4646389		

n	$\log\|S_n'(0{\cdot}9)\|$	$\log\|C_n'(0{\cdot}9)\|$	$\log\|E_n'(0{\cdot}9)\|^2$	n
0	$\bar{1}$·7935180	$\bar{1}$·8939430	0·0000000	0
1	$\bar{1}$·7049518	0·0069676	0·1104517	1
2	$\bar{1}$·1669123	0·9065070	1·8131581	2
3	$\bar{2}$·4140081	1·8467796	3·6935592	3
4	$\bar{3}$·5179920	2·8643957		
5	$\bar{4}$·5144285	3·9606240		
6	$\bar{5}$·4246666	5·1259264		
7	$\bar{6}$·2629681	6·3515890		

PART II.

Zeros of Bessel Functions of High Order.

The roots of Bessel functions $J_n(x)$ where the order n is large are of importance in the solution of physical problems. The table calculated by Bourget[1] gives the first nine roots of the six functions $J_0(x)$ to $J_5(x)$.

For large values of n, ρ_p, the pth root of $J_n(x)$ is approximately found[2] from

$$\rho_p = n\left(1+\frac{\lambda^2}{2}+\frac{3\lambda^4}{40}-\dots\right) \text{ where}$$

$$\lambda = \left[\frac{3}{n}\left\{\frac{(4p-1)\pi}{4}+\frac{5}{18(4p-1)\pi}\right\}\right]^{\frac{1}{3}}.$$

For the first three roots,

$$\rho_1 = n + 1{\cdot}857n^{\frac{1}{3}} + 1{\cdot}034n^{-\frac{1}{3}} \dots\dots$$
$$\rho_2 = n + 3{\cdot}245n^{\frac{1}{3}} + 3{\cdot}158n^{-\frac{1}{3}} \dots\dots$$
$$\rho_3 = n + 4{\cdot}382n^{\frac{1}{3}} + 5{\cdot}760n^{-\frac{1}{3}} \dots\dots$$

The table below was computed to six significant figures over a wide range of values of n by the method of successive approximation.[3] The colon is approximately equivalent to 5 : *e.g.* 11·0863(5) is the first root of $J_7(x)$, nearly.

First ten roots of $J_n(x)$.

$n=0$	1	2	3
2·4048 :	3·8317	5·1356	6·3801
5·5201	7·0156	8·4172 :	9·7610
8·6537 :	10·1734 :	11·6198 :	13·0152
11·7915 :	13·3237	14·7959 :	16·2234 :
14·9309	16·4706 :	17·9598	19·4094
18·0710 :	19·6158 :	21·1170	22·5827
21·2116 :	22·7601	24·2701	25·7481 :
24·3524 :	25·9036 :	27·4205 :	28·9083 :
27·4935	29·0468 :	30·5692	32·0648 :
30·6346	32·1897	33·7165	35·2186 :

$n=4$	5	6	7
7·5883 :	8·7715	9·9361	11·0863 :
11·0647	12·3386	13·5893	14·8212 :
14·3724	15·7001 :	17·0038	18·2876
17·6159 :	18·9801	20·3208	21 6415 :
20·8269	22·2178	23·5861	24·9349 :
24·0190	25·4303 :	26·8201 :	28·1912
27·1991	28·6266	30·0337	31·4228
30·3710	31·8117	33·2330	34·6371
33·5371	34·9888	36·4220	37·8387
36·6990	38·1598 :	39·6032 :	41·0307 :

[1] *Annales de l'Ecole Normale*, 3 (1866). Lord Rayleigh, *Theory of Sound*, Vol. i., Table B, p. 330.

[2] 'Calculation of the Roots of Bessel Functions.' *Phil. Mag.*, Sept. 1917, p. 193.

[3] 'The Roots of Bessel and Neumann Functions of High Order.' *Phil. Mag.*, July 1916, pp. 10 and 11.

Zeros of Bessel Functions of High Order—contd.

$n = 8$	9	10	15
12·2251	13·3543	14·4755	19·9944
16·0377 :	17·2412	18·4334 :	24·2691 :
19·5545	20·8070 :	22·0469 :	28·1024
22·9451 :	24·2339	25·5094 :	31·7334
26·2668	27·5837 :	28·8874	35·2471
29·5456 :	30·8853 :	32·2118 :	38·6843
32·7958	34·1543 :	35·4999	42·0679
36·0256	37·4001	38·7618	45·4121 :
39·2404 :	40·6285 :	42·0042	48·7264 :
42·4439	43·8438	45·2315 :	52·0172 :

$n = 20$	30	40	50
25·4171 :	36·0983 :	46·6484	57·1169
29·9616	41·0927 :	52·0161 :	62·8077
33·9887	45·4527	56·6583	67·6974
37·7728 :	49·5062	60·9447	72·1903 :
41·4130 :	53·3737	65·0122	76·4370 :
44·9576 :	57·1151	68·9293	80·5132 :
48·4342 :	60·7648 :	72·7360	84·4632 :
51·8600	64·3450 :	76·4580	88·3157
55·2465 :	67·8705	80·1127	92·0902 :
58·6020	71·3520	83·7127	95·8011

$n = 75$	100	200	300
83·071	108·836	211·029	312·577 :
89·430	115·739	219·514	322·192
94·839	121·575	226·613 :	330·191 :
99·770	126·870 :	232·986 :	337·358
104·401	131·824	238·907 :	343·988
108·821 :	136·535 :	244·502 :	350·233
113·084 :	141·066	249·848 :	356·185
117·225	145·453	254·998	361·903
121·266	149·725	259·986	367·429
125·225	153·900	264·838 :	372·793 :

$n = 400$	500	750	1000
413·813 :	514·859	766·974	1018·66
424·335	526·150	779·826 :	1032·76
433·065 :	535·502 :	790·444	1044·39
440·869	543·849	799·898	1054·73 :
448·074	551·545 :	808·601	1064·24 :
454·849 :	558·774 :	816·756	1073·15
461·296	565·645 :	824·496 :	1081·59
467·480	572·229 :	831·902 :	1089·66
473·447 :	578·578	839·033	1097·42 :
479·233 :	584·726 :	845·929 :	1104·93

Radiotelegraphic Investigations.—*Report of Committee* (Sir OLIVER LODGE, *Chairman*; Professor W. H. ECCLES, *Secretary*; Mr. S. G. BROWN, Dr. C. CHREE, Sir F. W. DYSON, Professor H. S. EDDINGTON, Dr. ERSKINE MURRAY, Professors J. A. FLEMING, G. W. O. HOWE, H. M. MACDONALD, *and* J. W. NICHOLSON, Sir H. NORMAN, Sir A. SCHUSTER, Sir NAPIER SHAW, Professor H. H. TURNER). *Drawn up by the Chairman.*

THE Committee on Radiotelegraphic Investigations was appointed at the Dundee Meeting in 1912, after a discussion upon the unsolved problems of wireless telegraphy which was opened by Professor J. A. Fleming. In the course of the following year the Committee decided to concentrate upon two or three of the principal large-scale phenomena, such as the variations in the strength of signals that have travelled long distances, and the nature of the electric waves that cause the telegraphic disturbances known as 'strays' or 'atmospherics.'

The Committee therefore organised a scheme of simultaneous widespread observations and proceeded to obtain the co-operation and support of Government departments, commercial companies, scientific workers, and amateurs. They drew up a series of forms suitable for distribution to all types of wireless observers in various countries.

During the winter of 1913-14 many forms were distributed, and many were duly filled and returned for analysis. The Committee's programme for the collection of observations was gradually extended to all parts of the English-speaking world and to several other countries. The Governments and the wireless companies of the United Kingdom, Canada, Australia, New Zealand, India, and the United States of America co-operated cordially. A special effort was made to obtain world-wide observations during the progress of the solar eclipse of August 21, 1914. But the outbreak of war sadly interfered with all these projects, especially in Europe. Nevertheless, sufficient information was collected to enable the preparation of the report presented at Manchester in 1915. The report discussed observations made in nearly every ocean and in many other parts of the globe, and is thought to constitute a landmark in the subject of large-scale radiotelegraphic investigation.

Since the conclusion of the War the chief item of work undertaken by the Committee was in connection with the solar eclipse of May 29, 1919. Observations were made in many countries, and a digest of the data collected formed the main substance of the report presented at Bournemouth in 1919.

Nevertheless, in view of the formation of the International Union of Radiotelegraphic Science, the Committee decided to discontinue their work, which, in fact, had embraced an area practically coincident with that contemplated by the new International Union. At the first meeting of the Union, held in Brussels on July 24, 1922, this decision was formally communicated to the Union, and arrangements were made for the transference to the Union of such of the Committee's records as have not yet been analysed, and also such as are likely to be of interest to the Union.

The expenditure of the Committee from the date of its inception in 1912 amounts to a total of 479*l.* 1*s.* 6½*d.*, and for this the Committee are indebted to the Caird Fund. Accounts and vouchers showing details of expenditure have been duly sent in.

The Committee is appreciative of the way in which its Hon. Secretary has conducted its affairs through an unexpectedly troublous time.

Colloid Chemistry and its General and Industrial Applications.

—*Summary Report of Committee* (Professor F. G. Donnan, *Chairman*; Dr. W. Clayton, *Secretary*; Dr. E. Ardern, Dr. E. F. Armstrong, Sir W. M. Bayliss, Professor C. H. Desch, Dr. A. E. Dunstan, Mr. H. W. Greenwood, Mr. W. Harrison, Mr. E. Hatschek, Mr. G. King, Professors W. C. Mc.C. Lewis *and* J. W. McBain, Dr. R. S. Morell, Professors H. R. Proctor *and* W. Ramsden, Sir E. J. Russell, Mr. A. B. Searle, Dr. S. A. Shorter, Dr. R. E. Slade, Mr. F. Sproxton, Dr. H. P. Stevens, Mr. H. B. Stocks, Mr. R. Whymper).

The publication of the Fourth Report was unavoidably delayed for many months, but it was published in August 1922 by H.M. Stationery Office. The following papers, eight of academic nature and six on industrial subjects, are included in the Report :—

Colloid Problems in Analytical Chemistry. By Prof. H. Bassett, D.Sc., Ph.D., D. ès Sc., F.I.C. (University College, Reading).

The fundamental colloid phenomena underlying most analytical operations are discussed, especially in connection with the formation, purity, filtration, and washing of precipitates. The colloid properties of the filter-paper, surfaces of vessels, &c., are also discussed. The Report includes a selection of important analytical determinations in inorganic and organic chemistry, illustrating the colloid principles under discussion.

Cataphoresis: The Motion of Colloidal Particles in an Electric Field. By Prof. E. F. Burton, M.A., Ph.D. (University of Toronto, Canada).

This paper is a summary of the main points in connection with cataphoresis, and includes recent work carried out in Prof. Burton's laboratory on the technique of cataphoresis.

Colloid Systems in Solid Crystalline Media. By Prof. Cecil H. Desch, D.Sc., Ph.D. (University of Sheffield).

The dispersion of solid particles throughout a crystalline solid, when the state of subdivision is sufficiently fine, introduces colloidal characteristics of interest in metallurgy and mineralogy—*e.g.* the hardening of steel and of certain iron-ferrous alloys, by quenching from a high temperature, and the colouring matters in crystalline minerals.

Molecular Attraction and the Physical Properties of Liquids. By Edwin Edser, A.R.C.Sc., F.Ph.S., F.Inst.P. (Minerals Separation, Ltd., London).

The law of molecular attraction derived in this paper may be stated as follows : Two molecules attract each other with a force that varies inversely as a power of the distance separating them, and this power must be higher than the fifth. In all liquids the result of analysing the experimental data is to indicate that the molecules attract each other inversely as the eighth power of the distance separating them ; mercury, however, is not in good agreement with this law.

It is deduced from this law that : (1) Of the energy which represents the surface tension of a liquid, 94 per cent. is located in the surface layer one molecule diameter in thickness, while the remainder is located at a greater distance from the surface ; (2) at a distance of one molecule diameter from the surface of a liquid the intrinsic pressure is 8.5 per cent. less than the maximum value in the interior of the liquid.

Membrane Equilibria. By W. E. Garner, M.Sc. (University College, London).

The remarkable potential differences occurring at cell surfaces—*i.e.* at the surfaces of membranes in contact with electrolytes—are discussed in the light of the theories due to Ostwald, Haber and Klemensiewicz, Loeb and Beutner, and Donnan. Special consideration is given to Donnan's theory based on thermodynamical treatment.

Disperse Systems in Gases. By W. E. Gibbs, D.Sc. (Chief Chemist, The Salt Union, Ltd., Liverpool).

The methods of formation of disperse systems in gases are studied—*e.g.* (1) by the condensation of a gas or vapour in the presence of suitable nuclei; (2) by the disintegration and dispersion of a liquid or solid. Those systems in which the particles are too large to exhibit Brownian motion at ordinary temperature and pressure are termed 'clouds,' while the more highly disperse systems are called 'smokes.'

The properties of these gas-solid and gas-liquid disperse systems are considered in the following order: (1) their mechanical properties—the concentration, the motion of the particles, and the degree of dispersion; (2) their optical properties—the absorption, reflection, refraction, and diffraction of light by the system; (3) their thermal properties—the absorption and radiation of heat; (4) their electrical properties—the electrical charges upon the particles, their behaviour in an electric field; (5) their chemical properties—the increased chemical activity of the disperse system, due to its high degree of dispersion.

The industrial applications are dealt with, and the paper concludes with a section on Chemical Warfare.

The Theory of Lubrication. By W. B. Hardy, M.A., F.R.S.

'A theory of lubrication must be founded upon a general theory of friction. No such general theory, however, exists, but in its place there is a theory of internal friction or viscosity which refers the resistance to fundamental forces between molecules, and a theory of external or superficial friction which, standing where Coulomb left it in 1781, accepts accidental inequalities of the surfaces as a sufficient cause.' The theories of Coulomb, Reynolds, and others are discussed, and a theory proposed, based on molecular orientation in surfaces, according to which one would expect 'that in general any good lubricant would be more strongly attracted by the bounding solid faces than a bad one. The expectation can only be a general one, because lubricating qualities depend not only upon the intensity of such attraction, but also upon the kind of orientation of the molecules produced by it, and the variation of potential energy in the lubricant and surface of the solid produced by traction.'

The Application of Colloid Chemistry to Mineralogy and Petrology. By Alexander Scott, M.A., D.Sc. (Central School of Pottery, Stoke-on-Trent).

The general principles of colloid chemistry are applied to important phenomena in mineralogy and petrology, and special attention is given to those rocks and minerals which exist in a colloidal form, or are derived from colloidal material. Weathering, cementation, adsorption, ore deposits, concretionary and banded structures, dendritic structures, igneous rocks, &c., are discussed as colloid problems. Rocks and minerals in large number are considered in much detail, and investigations mentioned which are needed to extend our knowledge of the mechanism of well-known natural phenomena.

Colloid Chemistry of Soap. Part II. The Soap Boiling Processes. By Prof. J. W. McBain, M.A., Ph.D., and Ernest Walls, M.A. (The University, and Broad Plain Soap Works, Bristol).

The first part of the report deals with the actual works practice of soap-boiling. The second part treats of the theory of soap-boiling, under the headings: Historical, general review, hydration of the fibres in soap curd. The third part of the report is entitled 'Application of the Theory,' and deals with saponification, graining and washing, fitting and settling, and the colour and hardness of soaps.

The Concentration of Minerals by Flotation. By Edwin Edser, A.R.C.Sc., F.Ph.S., F.Inst.P. (Minerals Separation, Ltd., London).

The fundamental scientific principles underlying the flotation of minerals are treated in detail—*e.g.* surface tension, contact angles, air films, the surfaces of liquids and solids, flocculation and deflocculation, frothing or foaming. The practice of flotation is described and an account given of the various hypotheses advanced to explain flotation. This is an extensive paper on a difficult but exceedingly important subject.

Colloids in Catalytic Hydrogenation. By E. F. Armstrong, D.Sc., F.R.S., and T. P. Hilditch, D.Sc.

Colloid phenomena obtain in the hydrogenation of gaseous or liquid organic compounds : (1) because of the surface at which interaction occurs; (2) when catalysts in the colloidal condition are employed. Catalysts may be colloidal sols or finely divided metals, non-supported or supported. The various physical and chemical conditions involved are considered, as well as the various theories of catalysis.

The Rôle of Colloids in Electrolytic Metal-Deposition. By Henry J. S. Sand, Ph.D., D.Sc., F.I.C. (Sir John Cass Technical Institute, London).

The influence of colloids in electrolytic plating solutions on the metal deposits obtained has been discussed under the headings : Phenomena produced by addition-agents; inclusion of the addition-agent in the metal-deposit, and its colloidal nature; adsorption and gold number of colloids in relation to their effectiveness as addition-agents; microstructure of deposits containing colloids; equilibrium potential of deposits containing colloids (transfer-resistance and polarisation); over-voltage produced by colloids in its relation to improved throwing-power of electrolytic baths, and to the sequence of electrolytic processes taking place at the electrodes; nature of process by which the final structure of electrolytic metal deposits is brought about; colloids at the anode; colloids in applied metal deposition.

Rubber. By Henry P. Stevens, M.A., Ph.D., F.I.C.

In the first report (1917) Dr. Stevens gave a summary of the chief papers on rubber. This bibliography is now extended and brought up to date, and the properties of rubber are discussed from the colloidal view-point. Vulcanisation is treated in detail.

Colloidal Fuels: their Preparation and Properties. By A. E. Dunstan, D.Sc., F.I.C. (Chief Chemist, Anglo-Persian Oil Co.).

This paper is a brief summary of recent work, particularly in the United States, on colloidal fuels.

Fuel Economy.—*Fifth Report of Committee* (Professor W. A. Bone,* *Chairman*; Mr. H. James Yates,* *Vice-Chairman*; Mr. Robert Mond,* *Secretary*; Mr. Robert Armitage, M.P., Mr. A. H. Barker, Professor P. P. Bedson, Dr. W. S. Boulton, Professor W. E. Dalby, Mr. E. V. Evans,* Dr. W. Galloway,* Mr. J. E. Hackford, Sir Robert Hadfield, Bart.,* Dr. H. S. Hele-Shaw,* Mr. D. H. Helps, Dr. G. Hickling, Mr. A. Hutchinson,* Mr. S. R. Illingworth, Principal G. Knox, Professor Henry Louis,* Mr. H. M. Morgans, Mr. E. Myers, Dr. J. S. Owens, Mr. W. H. Patchell,* Mr. H. Stafford Rayner, Mr. L. L. Robinson, Mr. A. T. Smith, Dr. J. E. Stead, Mr. C. E. Stromeyer, Mr. W. C. P. Tapper, Mr. W. Thorneycroft, Professor W. W. Watts,* *and* Mr. C. H. Wordingham *) *appointed for the Investigation of Fuel Economy, the Utilisation of Coal, and Smoke Prevention.*

* Denotes a member of the Executive Committee.

I. The Coal Situation.

Since the Committee last reported, the fuel situation in this country has been dominated by the effects of the coal crisis of 1921. The output of coal from mines in Great Britain during the year ending December 31, 1921, fell to 163 million tons, of which 24.66 million tons were exported, and a further 11 million tons (including about 133,000 tons of manufactured fuel) were consumed by steamers engaged in the foreign trade. There were also exported 443,565 tons of gas coke, 850,074 tons of manufactured fuel, and 292,648 tons of other sorts of coal fuel. Owing to the coal stoppage, there were imported into the country during the year (but chiefly during the three months April-July inclusive) altogether some 3.433 million tons of coal, and 136,424 tons of coke and manufactured fuel.

In its last Report the Committee drew attention to the effects of the then high coal prices upon the prospects of the iron and steel industry in this country; and as this particular industry reflects almost better than any other the effects of coal prices upon production, the following observations may appropriately be made upon the present situation.

The crisis of last year in the coal trade was the final cause of the severe depression which in the pig-iron and steel trade had gradually been setting in since the miners' strike in the autumn of 1920. The latter came upon the trade following a period of high prices and abnormally high demand, and precipitated a complete cessation of business and great difficulty in obtaining new orders. This was further accentuated by the general post-war economic conditions in the world's markets. At the same time on the Continent there was a cheapening in the cost of production, together with a serious drop in exchange values and pressure of the banks on the Belgian and French works to realise stocks, and in consequence the foreigner was enabled to deliver pig iron into Great Britain at a lower price than it could be made at home. Hence, whilst the pig-iron sales had been maintained fairly well up to the end of 1920, the first months of 1921 saw the importation of both pig iron and steel into Great Britain on a large scale, foreign pig iron being delivered into Scotland about thirty shillings per ton below the cost of manufacture in Great Britain. The coal crisis of the following months completed the disaster.

The combined effect of these various causes is well shown by contrasting the production of pig iron and steel in the United Kingdom in the years 1920 and 1921 respectively. During the year 1920 the output of pig iron was 8,034,700

tons, and of steel 9,067,300 tons; in 1921 these figures had fallen to 2,611,400 and 3,625,800 tons respectively. The following analysis of the corresponding productions for each quarter of 1921 is very instructive in this connection :—

	Pig-Iron Tons	Steel Tons
January-March	1,491,700	1,336,000
April-June	74,700	79,000
July-September	262,700	980,600
October-December	782,300	1,230,200
	2,611,400	3,625,800

On operations being resumed at the mines, production at iron and steel works restarted on a very restricted scale, and with the low prices then prevailing was carried on at a heavy loss. Despite subsequent reductions in wages due to the operation of sliding scales and other mutual arrangements, and in the excessively high railway rates, which gave manufacturers less relief than had been hoped for, there still seems little hope of further reductions in the present prices of pig iron and steel, the cost of fuel delivered to the works being still too high.

From a fuel-economy point of view the position is most unsatisfactory, because at present the prices of coke as compared with those of coking coals delivered at the works offer little inducement to iron and steel makers owning self-contained plants to start up the coke ovens connected with their steelworks, since coke can be bought at prices below the cost of making it in their own ovens, even after crediting the values of all the by-products produced in the process. This condition is entirely abnormal; and it is hoped that with improved general trade, freedom from industrial disputes and unrest, and an increase in demand, more blast-furnaces and steelworks will be put into operation, and that the demand for the raw materials will then tend to re-establish more normal conditions in the coal and coke trade, and thus restore the balance in favour of pursuing a policy which ensures the greatest fuel economy in the operation of plants, which is so desirable in the national interests.

II. Oil Fuel Supplies—Present and Future.

The Present Situation.—During the past year the Committee has had under consideration the important question of present and future supplies of oil fuel which are now needed for certain purposes (chiefly motor transport) for which at present coal cannot well be used. In this connection they have had the valued help of Mr. J. E. Hackford, who was co-opted on to the Committee on the nomination of the Institution of Petroleum Technologists.

As a first step towards the exploration of the subject, the Committee decided to collect authentic information as to the importation of petroleum products into the United Kingdom during each of the five years 1917-1921 inclusive. For purposes of reference the data so collected have been analysed and tabulated as follows :—

TABLE I.

Imports of Petroleum Products into the United Kingdom for the years 1917 to 1921 inclusive.

(*Tons.*)

	1917	1918	1919	1920	1921
Crude Oil .	1·65	—	31,388·0	17,417·1	426,151·0
Lamp Oil .	456,995·4	528,644·6	544,185·0	574,828·2	536,575·6
Motor Spirit	422,030·9	584,724·3	602,324·8	629,511·5	768,287·5
Lubricating Oil .	351,188·0	409,095·2	263,332·0	426,656·0	204,059·8
Gas Oil .	120,400·0	149,375·0	115,511·1	206,018·4	304,736·6
Fuel Oil .	1,835,759·1	3,515,020·8	1,105,855·0	1,425,045·6	2,255,822·2

TABLE II.

Imports of Petroleum Products for the year ending December 31, 1921.

[Customs *corrected* returns in tons, to the nearest decimal place.]

Origin	Motor Spirit	Kerosene	Lubricating Oil	Fuel Oil	Gas Oil	Crude Oil	Others	Total
U.S.A.	414,165·7	440,410·6	192,185·3	820,610·4	292,181·3	7,533·9	300·5	2,167,387·7
Mexico	68,430·9	59,182·7	13,011·4	1,129,758·3	996·5	61,982·6	—	1,333,362·4
Dutch E. Indies	109,211·1	—	0·5	8,943·7	—	—	—	118,155·3
Dutch W. India Islands	5,649·3	—	—	—	—	—	—	5,649·3
Poland	—	—	19·8	—	2,184·0	—	—	2,203·8
Peru	9,092·9	—	—	—	—	—	—	9,092·9
Roumania	9,426·7	20,900·5	—	—	—	6·0	—	30,333·2
Russia	—	4,771·3	1,372·0	—	—	—	—	6,143·3
Persia	117,654·7	—	—	178,263·9	—	350,650·4	—	646,569·0
Other Foreign Countries	0·2	70·3	1,465·5	31,731·1	374·8	13·2	16·6	33,671·7
TOTAL from Foreign Countries	733,631·5	525,335·4	208,054·5	2,169,307·4	295,736·6	420,186·1	317·1	4,352,568·6
British India	8,458·5	—	5·1	—	—	—	—	8,463·6
British W. India Islands	6,870·7	5,635·6	—	85,483·8	—	5,964·8	34·8	103,989·7
Canada	—	—	—	—	—	—	8·8	8·8
Egypt	6,314·4	5,605·1	—	1,042·0	—	—	—	12,961·5
Sarawak	10,571·8	—	—	—	—	—	—	10,571·8
Straits Settlements	2,439·5	—	—	—	—	—	—	2,439·5
Other British Possessions	—	—	0·2	—	—	—	4·4	4·6
TOTAL from British Possessions	34,654·9	11,240·7	5·3	86,525·8	—	5,964·8	48·0	138,439·5
TOTAL	768,286·4	536,576·1	208,059·8	2,255,833·2	295,736·6	426,150·9	365·1	4,491,008·1
Percentage from Countries outside the Empire	95·5%	97·9%	100·0%	96·1%	100·0%	98·6%	86·9%	96·9%

The Committee then proceeded to inquire into the origins of our present imported oil-fuel supplies, and for that purpose the statistics for the year ending 1921 were analysed (see Table II.) so as to discriminate between the various countries of origin, and also to show how much of our total imported supplies of petroleum and petroleum products are being drawn from within the Empire itself.

The Committee considers it important that attention should be drawn to the fact that, inasmuch as the home production of shale oil is now very small,[1] owing to the relatively low cost of production of imported petroleum, we are at present dependent almost entirely on countries outside the Empire for the supplies of natural petroleum and petroleum products, a most undesirable and dangerous state of affairs from every point of view.

The Gas and Coking Industries as Producers of Motor Spirit.—It would appear that the only practicable way in which future home supplies of motor spirit and fuel oils can be extended is by the carbonisation of bituminous coals at temperatures between 600° C. and 1200° C. By carbonising suitable British coals at high temperatures (1000° C. to 1200° C.) in gas retorts or by-product coke ovens there can be obtained between 3 and 6 per cent. of the weight of the dry coal as anhydrous tar, and between 0.75 and 1 per cent. of its weight as refined benzole (motor spirit). It should be borne in mind, however, that, inasmuch as more than half of the coal so carbonised is for the production of hard metallurgical coke, the total production of motor spirit and tars by these methods depends very largely upon conditions in the iron and steel industries which fluctuate and at the present time are exceedingly bad. Supposing, however, that pre-war prosperity were restored to the iron and steel industries, a total high temperature carbonisation of some 40 million tons of coal per annum might reasonably be expected. Taking 1 per cent. of its weight as a probable outside limit for the ultimate production of refined benzole, the total potential production of the latter would not exceed 400,000 tons per annum, which is very little more than half the tonnage of the motor spirit actually imported into the country in the year 1921. Moreover, of this potential supply nearly half would have to be drawn from the gasworks of the country, and it is at least problematical whether it would pay the gas companies to extract the benzole from their gas at present prices. Indeed, taking the present relative prices of gas and benzole, as well as the costs of recovering benzole from coal gas, it is probable that the cash value of the potential heating power of benzole is greater when left in the gas than it is when extracted therefrom and (after subsequent refining) sold as motor spirit.

Low Temperature Carbonisation of Coal as a Future Source of Oil Fuels.—Therefore, it has to be recognised that the most promising internal supply of both motor spirit and fuel oils lies in the direction of the low-temperature carbonisation of coal, provided that methods can be devised for same which are sound both from the technical and the commercial points of view. A really successful solution of this problem is greatly to be desired, not only for the aforesaid reasons, but also because it would be a great factor in the abolition of the smoke nuisance, especially from domestic fireplaces. The Committee has paid close attention to the recent developments with regard to this matter which are taking place in this country. In this connection attention may be drawn to :—

1. The description of the operation of the experimental plant at Barugh, near Barnsley, as published in *Engineering* of October 28, 1921.
2. The Conference on low-temperature carbonisation which was held at Cardiff on April 20, 1922, under the auspices of the South Wales Institute of Engineers.
3. The paper read on April 3 last on 'The Influence of Structure on the Combustibility and other Properties of Solid Fuels,' by Messrs. E. R. Sutcliffe and E. C. Evans, before the London Section of the

[1] Thus in the year 1920 the amount of oil-shale mined in Great Britain was 2,840,859 tons, from which it may be estimated that no more than about 227,000 tons of oil would be produced.

Society of Chemical Industry (*Journ. Soc. Chem. Ind.*, Vol. xli., p. 196 T.).

4. The Report on Low Temperature Carbonisation recently issued by the Fuel Research Board (H.M. Stationery Office, 1922).

Taken together, these publications give a fairly comprehensive view of the present position of affairs in regard to the low-temperature carbonisation in this country. The Committee is in general agreement with the view recently expressed by the Fuel Research Board that, although we have not yet reached the stage when a final answer can be given to the question whether or not it will be possible to establish on sound industrial lines a new industry based on the carbonisation of the tens of millions of tons of coal per annum which are at present being consumed in the raw state in this country, yet as the result of the pioneering work which has been done during recent years by various organisations such knowledge and experience has been gained as affords some ground for the expectation that we are approaching a conditional solution of the matter

There still seems to be some difference of opinion as to whether from the commercial point of view it will be better to carbonise at temperatures round about 600° C. or at somewhat higher temperatures (say, 700° C. to 750° C.), but this may be regarded as a minor issue. It seems now to be established, as the result of fairly large scale trials, that the average yields of the various products now obtainable by carbonising suitable British bituminous coals at a temperature of 600° C. will amount (on the weight of the dry coal carbonised) to about 7.5 per cent. of tars, and about 2.5 gallons per ton of motor spirit, besides about 3,500 cubic feet per ton of a rich gas of a gross calorific value (say) of about 800 B.Th.Us.,[2] and a 70 to 80 per cent. residue of smokeless semi-coke.

Seeing that the cash value of the semi-coke residue far exceeds that of all the other products put together, and also that the price of fuel oil in this country will probably also be determined by circumstances beyond our control, it seems as though the ultimate prospects of a low-temperature carbonisation industry will depend upon the price which the public will be willing to pay for a smokeless domestic fuel. There can be little doubt but that such a fuel, properly manufactured, is a very suitable one for domestic consumption; it burns freely and smokelessly, and also, according to Dr. Fishenden's recent experiments (*vide* Fuel Research Board Special Report No. 3), it has a greater radiant efficiency than either coal or high-temperature coke. Its general adoption, however, will probably depend upon two other conditions being fulfilled. Firstly, it must be prepared and distributed in a form which will allow of its being freely handled without undue disintegration. Secondly, if its ash content could be cheaply reduced to a low figure by subjecting the coal to some washing process such as froth flotation or the like before it is carbonised, its attractiveness as a domestic fuel would be undoubtedly greatly increased. Indeed, it seems possible that public opinion might soon be educated to regard with favour well-manufactured smokeless semi-coke of better combustibility and of smaller ash content than raw coal.

From this point of view, the recent work of Sutcliffe and Evans upon the influence of porosity on the combustibility of solid fuels (*loc. cit.*) is of interest, inasmuch as it draws attention to a factor whose significance is not always sufficiently recognised in fuel technology. These authors consider the porosity of the cell walls of a carbonised fuel to be extremely important in determining its combustibility. Their suggestion that, before the coal is carbonised for the production of a free-burning smokeless domestic fuel, it should be finely pulverised and briquetted by pressure without the use of a binder, in order that the thermal availability of the resulting semi-coke may be raised to a higher level, deserves further investigation from both the technical and commercial standpoints, especially if it could be found practicable to

[2] The Fuel Research Board's Report gave figures which would average about 1,000 B.Th.Us., but the Committee has thought it better to adopt the more conservative estimate of 800 B.Th.Us. here.

combine it with some preliminary washing process in order to reduce the ash content of the semi-coke.

Supposing that further technical developments result in the establishment of a low temperature carbonisation industry for the manufacture of a smokeless domestic fuel, it is of interest now to forecast the effect of such developments upon the future home supplies of motor spirit and fuel oils. It may be provisionally assumed that the amount of coal which could be so carbonised to supply domestic fuel requirements would be at least 40 (and possibly even 50) million tons per annum. Taking the former of these two figures, this would mean a possible production of about 100 million gallons (or about 350,000 tons) of motor spirit, and 600 million gallons (or about 2,750,000 tons) of anhydrous tars (fuel oils).

Present Tendencies in the Use of Oil Fuel.—Before the advent of the internal combustion engine, the term 'fuel oil' was restricted to an oil intended to be burnt in furnaces and the like. In the early days of oil fuel practice, comparatively light oils were used. They were 'atomised' and injected into the boiler furnace or the like by means of steam or air, and the burners used were often of a crude and unsatisfactory character. In recent years, however, more attention has been given to the proper design of such burners, and to the more effective combustion of the fuel oil. The consumption of air or steam by the oil burners has been considerably reduced, and, as they have attained a higher degree of mechanical efficiency, heavier oils have been successfully used in them. Methods have now been devised whereby heavy oils may be sprayed into the furnace under pressure alone, without the aid of either steam or air; and, therefore, it has become possible for heavy petroleum residues, such as asphaltum, to be employed as first-class fuels for land installations. A similar tendency has recently been manifested in the use of fuel oil in internal combustion engines. Until quite recently petroleum refiners prepared a very light fuel oil for these engines, but mechanical research is now being directed in order to render them capable of burning even the heaviest oils. Progress in this direction has been greatly helped by scientific investigations upon the spontaneous ignition temperatures of fuel oils. The outstanding question to-day is how to adapt a heavy fuel oil for use in low compression internal combustion engines; this is now receiving the close attention of scientific investigators, and it may be hoped that as a result of their work the public will in due course be enabled to use heavy fuel oil in the place of motor spirit.

III. The Chemistry of Coal.

Progress towards the solution of the problem of the constitution of coal substance can be recorded, notwithstanding the magnitude and complexity of the problem. The difficulties arise in no small measure from the absence from the products of such researches of bodies with crystalline habit or other well-defined physical characters by which the chemist is accustomed to identify the compounds he isolates. Still, the literature shows the subject has attractions for not a few chemists who, employing various methods of attack, seek to obtain information as to the nature of the multifarious compounds which go to the make-up of coal. As, however, different investigators select for their study coals of varying origin and of different classes, it is not always easy to compare the results obtained.

The work of Clark and Wheeler (*Trans. Chem. Soc.*, 1913, 103, p. 1704), combining the application of solvents with the study of the action of heat upon the extracts of the coal so obtained, has undoubtedly given much useful and valuable information. The results, however, still leave open to conjecture and theoretical explanation the true nature of the components of the several fractions. The classification of the coal components by Clark and Wheeler based upon the pyridine-chloroform treatment is too facile; nor could it be expected to provide material for a complete explanation of the properties of coal.

The breaking down of a bituminous coal by treatment with a mixture of pyridine and amyl alcohol yields an extract from which, by successive use of ether and light petroleum, Bone, Pearson, Sinkinson, and Stockings (*Proc. Roy. Soc.*, A, vol. 100, 1922, p. 582) have succeeded in obtaining (1) a non-resinous wax-like substance, (2) a resin, to which the formula $C_{31}H_{32}O_3$ is assigned,

(3) a portion, insoluble in ether, consisting of non-resinous material, partially dissolved by alcoholic potash, and this they designate as 'humic substance.' The authors are satisfied that these humic substances are not 'resinic' but 'cellulosic' in origin. The influence of these several fractions on the coking of a coal has been studied, with the result that, whereas the said resin is in part responsible, the main cause of the coking propensities was shown to be a series of substances of 'humic' type which are soluble in chloroform but not in ether, and whose fusion temperatures are below those at which they undergo rapid decomposition.

The acidic substances extracted by alkalis from the aforesaid humic bodies are precipitated by acids from these solutions, as bulky, dark-coloured, opaque jellies. These jellies on drying form black, brittle, lustrous, and structureless masses, with conchoidal fractures, suggestive of the material forming bright coal which Stopes has styled 'vitrain.' The consideration of these facts has led Bone and his co-workers to suggest that 'bright coal' may have originated in a colloidal gel.

In this connection attention may be directed to a like conclusion arrived at by Dr. J. A. Smythe in 1906, in a paper read before the University of Durham Philosophical Society[3] which dealt with certain Peaty Deposits from a Pit-Fall at Tantobie, County of Durham. Amongst the substances described is a black jelly-like body, which Smythe styled 'black-stuff,' and this he showed by its composition and behaviour to solvents, notably to pyridine, suggests a similar relationship to bright coal.

It has long been recognised that bituminous coals contain three easily distinguishable components, which until recently had usually been designated (*a*) 'mother of coal' or 'mineral charcoal,' (*b*) 'dull hard coal' (Ger. = 'Mattkohle'), and (*c*) 'bright coal' (Ger. = 'Glanzkohle'), respectively, the last named being a structureless, lustrous substance with a conchoidal fracture. Recently Stopes (*Proc. Roy. Soc.*, B. 90 (1919), p. 470) proposed new names for them, namely (*a*) *fusain*, (*b*) *durain*, whilst (*c*) is termed by her either *clarain* or *vitrain*, according as it does or does not contain recognisable plant tissues and structures. In putting forward these proposals Stopes recognised that none of the four said ingredients (with the possible exception of vitrain) are either homogeneous or chemical molecular units; also that they do not even approximately represent the crystals in a petrological section of a rock. Provided that such qualifications are kept clearly in mind, and that it be realised that clarain may prove to be merely vitrain in which plant structures occur in suspension, the Committee sees no great objection to the provisional substitution of the proposed new names for the older ones, regarding the matter more as one of convenience than as involving any new principle.

The Committee also feels that the growing practice among coal-chemists to use the terms *alpha*, *beta*, *gamma*, &c., to designate the several components obtainable from the coal substance by fractional extraction of it by means of various solvents, is one which, unless regularised in some definite way, is likely to lead to much confusion and obscurity, to the detriment of progress. It is obvious that coal may be 'fractionated' in as many different ways as there are suitable solvents and modes of applying them; and therefore unless, as the results of some particular treatment or procedure, components of a reasonable degree of purity and well-defined properties are isolated, it is undesirable that definite names should be assigned to them, as though they were the actual chemical constituents of the coal instead of unknown mixtures of them. The Committee, therefore, would suggest that the time has come when chemists should agree in conference upon some common plan of labelling such 'coal fractions' which, whilst recognising them to be such, shall also in some way indicate how they have been obtained.

Supposing, for example, that a particular investigator extracts a coal with two solvents A and B, he might designate the fraction which is insoluble in both as the α AB fraction, whilst β AB might be used to denote the fraction which is soluble in A but not in B, and the γ AB that which is soluble in both A and B, assuming all solvents to be used at their respective boiling

[3] *University of Durham Philosophical Society Proceedings*, Vol. 2, pt. 6.

points at atmospheric pressure. Thus, a fraction termed the '*α pyridine-chloroform*' fraction would mean the residual insoluble portion of a coal after successively extracting it with pyridine and chloroform at their respective boiling points; the '*β pyridine-chloroform*' fraction would be that portion of the pyridine extract which is insoluble in chloroform; whilst the '*γ pyridine-chloroform*' fraction would mean the portion of the coal which is soluble in both pyridine and chloroform, and so on. In cases where the coal is extracted at some particular temperature other than the boiling point at atmospheric pressure of the solvent employed, the actual temperature employed might be designated by putting it in small type above the name of the solvent, thus '-benzene $^{120°}$' denoting that benzene had been used at 120° C.

IV. Brown Coals and Lignites.

Although Great Britain itself is almost destitute of sub-bituminous coals and lignites (the Bovey Tracey deposit in Devonshire being the only important one in this country), the problem of using them efficiently is of great importance to several of the Dominions, and especially so to Australia, Canada, and India. Of the total estimated Canadian reserves, amounting altogether to 1,234,268 million tons, no less than 1,072,627 million tons are of a sub-bituminous lignitic class occurring in the upper cretaceous formations of the province of Alberta, whilst in the neighbouring province of Saskatchewan there are both cretaceous and tertiary lignite reserves amounting to 59,812 million tons. The Dominion Government has set up a Lignite Utilisation Board for the purpose of investigating the best means of utilising these resources, and it is hoped that the approaching meeting of the Association at Toronto in 1924 will afford an opportunity of discussing the problem in all its bearings.

In Australia, the provincial Governments of Victoria, South Australia, and New South Wales are all interesting themselves in the utilisation of their brown coal and lignite resources. Of these, the celebrated Morwell deposits in the Gippsand district of Victoria, which are of phenomenal thickness without parallel elsewhere in the world, are of great scientific interest, as well as of economic importance for the future of Australia. It has been estimated that within an area of 50 square miles in the Latrobe Valley, and within 1,000 ft. of the surface, there are 31,144 million tons of the coal. A bore-hole put down near Morwell disclosed no fewer than seven beds of brown coal within 1,000 ft. of the surface, of a total thickness of 781 ft., the individual seams (taken in order from the surface) running 29 ft. 8 in., 25 ft. 8 in., 23 ft., 227 ft. 10 in., 265 ft. 6 in., 166 ft., and 43 ft. 8 in. respectively. So far as they have been examined, they were reported by the Victorian Advisory Committee on Brown Coal, in 1917, as consisting of 'a matrix of earthy brown coal, with sporadic inclusions of lignite . . . the matrix consists of pollen grains, spore cases, and decomposed vegetable matter. . . . The coal varies in colour between yellowish brown and black, but it always pulverises to brown powder.' The raw coal usually contains about 50 per cent. of water; the *dry* coal contains: Carbon = 62.5, Hydrogen = 4.85, Nitrogen = 0.45, Sulphur = 0.20, Oxygen = 28.00, and Ash = 4.00 per cent. Its gross calorific value is about 5,600 K.C.Us. per kilogram.

In the year 1917 the Advisory Committee appointed by the Victorian Government to investigate the possibilities of generating electric power on a large scale from the Morwell coal reported that, notwithstanding its low grade, power could be more cheaply generated from it for the City of Melbourne than from black coal imported from New South Wales. It has been officially estimated that the cost of producing raw Morwell coal at the mines will not exceed 2*s*. 6*d*. per ton. The Victorian Government has already authorised the expenditure of 6,000,000*l*. upon the development of the Morwell coal deposits in the expectation that by the year 1924 electrical energy from thence will be supplied, not only to the City of Melbourne, but also throughout the whole State of Victoria. It has been calculated that the cost of such energy at the mine will be as low as 2*l*. 17*s*. 6*d*. per horse-power per annum, and that it can be sold profitably to manufacturers throughout the State at an average price of 4*l*. 8*s*. per horse-power per annum. Large-scale steam trials are, or have been, in progress with a view to ascertaining how the coal may best be

burnt under boilers, and a large electric power-station scheme at Morwell is rapidly materialising, and a large order for water-tube boilers in connection therewith has recently been placed in this country.

The problem of how such low-grade fuel as brown coals and lignites can be most efficiently burnt in boilers has therefore become one of great importance. It is obvious that a prime condition of efficient combustion is that coal shall be dried before being burnt; and as this drying can be effected at the expense of some of the sensible heat in the waste gases from the boiler, provided that they contain not less than 10 per cent. of carbon dioxide, such a drying operation may be cheaply carried out as an integral part of the boiler operation. Whether or not, as an addition to such drying operation, the coal should be subjected to a preliminary low-temperature carbonisation, using the residue therefrom as the boiler fuel, is a matter for future investigation to decide. In this connection attention may be drawn to the recent discovery made by Bone (*Proc. Roy. Soc.*, A, vol. 99, 1921) whilst investigating Morwell brown coal, Saskatchewan and other typical lignites :—

(*a*) that there is for each particular brown coal or lignite a certain definite temperature limit (usually between 300° C. and 400° C.) up to which it may be heated (in the *dry* state) so as to effect a considerable chemical condensation in its cellulosic or humic constituents, with simultaneous expulsion therefrom of steam and carbon dioxide, together with a small but variable proportion of carbonic oxide; and that such chemical condensation is unaccompanied by any other change productive of either hydrogen or hydrocarbons;

(*b*) that, by means of such condensation, substantially the whole of the potential energy of the fuel may be correspondingly concentrated by suitable heat treatment (within the prescribed temperature limit) in the resulting carbonaceous residue, which may therefore be burnt with greater calorific intensity than the original coal; and

(*c*) that, accordingly, such treatment constitutes a possible means of 'up-grading' brown coals and lignites generally, thus improving their fuel values.

V. Domestic Heating and Cooking Appliances.

A good deal of valuable investigation work has recently been done by persons not actually engaged in the industries concerned in the direction of testing the efficiencies of domestic heating and cooking appliances, with results of considerable interest in connection with the important question of fuel economy in our homes, where are burnt not only some 40 million tons of coal per annum, but also the bulk of the gas and some of the coke sent out from the country's gasworks.

In the first place, physiological research has emphasised the intimate connection, from the point of view of health and comfort, between the cognate problems of heating and ventilation in regard to domestic apartments. Moreover, the introduction of the Kata-thermometer has placed at our disposal a new method for estimating the 'cooling power' of the air, which has been shown to be a governing factor in regard to what may be termed 'comfort conditions' in living rooms. Also, the physiological value of radiation from a red-hot incandescent surface, as distinct from convected heat, has become to be more clearly recognised than ever before. Indeed it may be said that the more nearly the conditions under which our living rooms are warmed and ventilated approach those of a warm summer's day—a cooling breeze blowing round the head, the varying sunshine warming one side of the body, and the warm ground for the feet—the more comfortable and healthful will they be. The desirability of such conditions, which may be contrasted with the warm air of rooms heated by convection from steam coils, probably explains the Englishman's decided preference for the radiation from an open fireplace during our dreary British winters over the various forms of central heating which are favoured in America and other countries where the winters are colder but brighter. Therefore, having regard to the character of British winters, the estimation of the 'radiant efficiencies' of domestic fires is of predominant importance.

The 'radiant efficiency' of a modern gas fire may be said to be about 50 per cent. on the *net* calorific value of the gas burnt therein; moreover, experiments made under Professor W. A. Bone's direction at South Kensington have proved that, within wide limits, such radiant efficiency is independent of the chemical composition and calorific value of the gas burnt, provided that the number of calories by combustion developed per hour is kept suitably constant for the particular size of fire. Gas fires are now available which are capable of ventilating rooms quite as well as an open coal fire; and it may be taken for granted that both these appliances are capable of providing a healthful source of radiation for the warming of living rooms without unduly heating the atmosphere thereof. With regard to electric radiators, whose radiant efficiency may be as high as 75 per cent., while these are very portable and therefore convenient for placing where the heat is required, they do not directly ventilate an apartment.

The recent determination by Dr. Margaret Fishenden, for the Manchester Corporation Air Pollution Advisory Board, of the radiant efficiencies of coal, coke, and semi-coke when burnt in open fire-places (Fuel Research Board Special Report, No. 3) are of considerable interest. Her experiments have shown, not only that such efficiency is much greater than has generally been supposed, but also that there is not so much difference as might be thought between the efficiencies of different grates.

Working with a number of coal-fired open grates, including what were supposed to be the best and the worst types, the radiant efficiency in all cases was found to lie between 20 and 24 per cent. of the heat of combustion of the coal actually burnt during each test. When *dried* coke was used as fuel, radiant efficiencies up to 25 and even 28 per cent. were obtained; the values were, however, materially diminished when wet coke was used. Tests made with the low-temperature carbonisation 'semi-coke' gave radiant efficiencies of up to 33 per cent. in a grate which with ordinary coal gave 25 per cent. It is also to be noted that the tests showed that, within the limits likely to be encountered in practice, with domestic fire-grates the radiant efficiency was found to be independent of the draught intensity, and also confirmed previous experience that treatment of the fuel with preparations (consisting mostly of common salt), which are sometimes advertised as doubling the value of a ton of coal, had no appreciable effect upon its radiant efficiency. Altogether, then, Dr. Fishenden's investigations may be said to have gone far to remove from the open fire-place the stigma of gross inefficiency.

The Fuel Research Board have also published some work by Dr. Fishenden which deals with the efficiency of kitchen appliances (Fuel Research Board Technical Paper No. 3: 'The Efficiency of Low-temperature Coke in Domestic Appliances'), and, in addition, one of the members of our Committee (Mr. A. H. Barker) has also devoted considerable time to the difficult question of the determination of the efficiency of domestic cooking appliances. A memorandum by him on the subject is appended to this Report, the results of his experiments having been published in detail by the Fuel Research Board (Special Report No. 4).

Dr. Fishenden has concerned herself with the determination of the radiant efficiencies of the ranges examined, as well as their efficiency in the production of hot water, whereas Mr. Barker has concerned himself generally with the question of the efficiency of ranges when functioning for the production of hot water or for cooking. The latter Report also touches upon the question of gas and electric cookers, but the Committee does not propose to comment upon this aspect of the case as it would appear to require further investigation.

Whilst perhaps it would be premature to express any final opinion as to the precise significance of the results obtained, in view of the fact that the work is from the experimental point of view in its infancy, and that rather marked differences in efficiencies are cited in the two Reports, due probably to differences in the type of range tested, the Committee desires to call attention to the very great interest of the work at the present juncture, and to the desirability of the inquiry being continued.

The combination of several different functions, namely, of a fire-heated oven, a fire-heated hotplate, a fire-heated boiler, and also a fire to warm the kitchen, would appear to involve heavy fuel consumption, as compared with the fuel

required for appliances designed to perform these functions separately. Thus, for example, Dr. Fishenden obtained a water-heating efficiency of 17 per cent. from an old-fashioned open kitchen range, whilst a later type of independent boiler—which also functioned as an open fire—gave an efficiency of 35 per cent. for water heating, and, in addition, an open fire radiation of 7 or 8 per cent. Moreover, Mr. Barker's tests yielded hot-water efficiencies varying from 7 to 17 per cent. in kitchen ranges, as against efficiencies of 40 to 50 per cent. obtainable with separate hot-water supply apparatus.

Mr. Barker's work indicates also that the existing designs sacrifice fuel economy to convenience in providing an exposed hotplate adjacent to the oven, and that the fuel consumption for oven cooking may be reduced by lagging the hotplate. It is difficult to assess the value of the house heating done by the present-day kitchen range, but any detailed review of the subject should make some endeavour to assess its value.

In the Committee's Report in 1916 it was stated that 'the whole question of the domestic use of fuel bristles with difficulties and complications . . . the solution or recommendation of particular means or apparatus for domestic heating cannot always be based simply upon the question of thermal efficiency, because it also involves considerations of a physiological and even of a psychological character. In the vast majority of houses inhabited by the artisan population, the kitchen fire or stove is the only place in the house where fuel is burned.' In addition, it might be added that in the latter type of house prime cost often becomes the determining factor, and sacrifices of efficiency have to be made to ensure a small capital outlay.

The Reports under review appear to the Committee to justify a reconsideration of the factors underlying the design of solid fuel types of domestic appliances with a view to determining whether improvement in fuel economy can be obtained without either an unreasonable sacrifice of convenience or an excessive addition to the cost of production. In very many cases the actual dweller has had no say in the selection of the kitchen range, nor has he the means or the facilities for replacing such as may have been provided for him.

With regard to the existing types of combined range, it is felt that attention should be called to one point in Mr. Barker's Report, namely, that the CO_2 content of the flue gases did not exceed 5.5 per cent. in the best ranges tested, and that in several cases it was much below this, suggesting that in some cases sufficient attention has not been paid to the regulation of the air supply.

The Committee would suggest the following three points for more general consideration: (1) The general adoption of means to reduce the excess of air drawn through the system by so enclosing the fire that air does not get to the combustion chamber otherwise than through the fire grate, although retaining a feature which characterises existing appliances in providing means to enable an open fire to be obtained for kitchen heating when the other functions of the range are not required; (2) the use of effective lagging of oven doors which is not generally adopted at present; (3) the desirability of removing the ordinary hot-water boiler from the range and the substitution therefor of an independent boiler, separately fired; at present it would appear that such combinations are only provided in a limited number of middle-class houses, or in the case of very large ranges.

VI. Steam Raising and Power Production.

The Committee desires to call attention to the great need there is for some more systematic effort on the part of steam users to improve the present admittedly unsatisfactory state of boiler practice throughout the country, especially in the direction of educational provision for the better training of stokers and power-station superintendents. Notwithstanding the greater attention which is nowadays paid to the subject of 'efficiency' in some of the larger steam-raising installations and power stations, there still exists in far too many cases a lamentable disregard for the elementary principles of good boiler management. Indeed it may be doubted whether, taking the country as a whole, the average efficiency of steam raising exceeds 60 per cent. on the calorific value of the coal burnt, whereas if scientific operations replaced rule-of-thumb working it might be raised to 75 per cent., with consequent great saving in fuel.

The most frequent and serious cause of avoidable heat-wastage in current boiler practice arises from the fact that unnecessarily large excesses of air are usually drawn through the system owing to sheer neglect of some of the most obvious precautions. With good management it should be possible, by careful damper regulation and maintaining a correct depth of fire, to burn completely an average quality of steam coal with no greater excess of air than would give about 12 per cent. of CO_2 (without appreciable quantities of CO) in the chimney gases; but far too frequently as much as twice such minimum excess of air is drawn through the system. It is not sufficiently realised how highly important to fuel economy are the proper regulation of the draught by dampers, the correct proportioning of grate area to the quantity and size of the coal burnt, and the avoidance of inleakage of cool air into the boiler setting by keeping the brickwork in good repair and well pointed, and efficiently caulking the joint between the brickwork and boiler shell. Proper attention to such elementary points would reduce the 'sensible heat' lost in the chimney gases to 18 per cent. of the total calorific power of the coal burnt, whereas neglect of them often means twice such loss.

Whilst the more general use of indicating and recording apparatus may be recommended as the best automatic aids to good management, yet unless these are supplemented by intelligence and watchfulness on the part of both stokers and boiler-house superintendents they will not avail much or may be actually misleading. The Committee, therefore, desires to impress upon both manufacturers and the public education authorities the need there is not only of better boiler-house supervision but also of the better instruction and training of the boiler-house personnel. It cannot be urged too often upon steam users that considerable economies can be effected with existing plant and appliances, provided that they are run under the skilled supervision of properly trained men. In the case of large boiler installations it will usually pay to put them under the control of a well-trained fuel technologist, and there are several institutions in the country where such men are being scientifically as well as practically trained. Local education authorities could effectively help fuel economy by instituting in the various technical schools throughout the country properly organised classes for the instruction of stokers and the lower grades of boiler-house attendants. In addition to such classes, the institution in some of the larger centres of more advanced and specialised lectures, with opportunities for discussion, upon combustion, heat transmission, and boiler management generally for boiler superintendents and engineers, would undoubtedly be of great advantage.

In connection with the production of electric power by Public Utility Undertakings, the Committee would point out that the Electricity Commissioners could render a great national service if, in their annual returns, they would publish such financial and detailed technical data as are necessary to show the actual fuel consumptions and total cost of production per unit of output in the various individual power stations throughout the country. The present annual returns are not sufficiently detailed for this purpose, and in particular do not show the circumstances (such as load factor) under which the current is produced. It would undoubtedly stimulate healthy competition, promote fuel economy, and reduce costs if the individual power stations were required to furnish for publication all the necessary technical data to enable fair comparisons to be made.

VII. Smoke Abatement.

During the past year the Departmental Committee appointed by the Ministry of Health 'to consider the present state of the law with regard to the pollution of the air by smoke and other noxious vapours, and its administration, and to advise what steps are necessary and practicable with a view to diminishing the evils still arising from such pollution,' has issued its Final Report. After full consideration of the matter, it was not thought practicable at present to propose legislation dealing with smoke from private dwelling-houses, although it was estimated that as much as 2½ million tons of potential fuel in the form of soot escape annually into the atmosphere from domestic fire-places, as against only 500,000 tons from factory chimneys, which latter seems a very low figure. It

was, however, recommended that the Central Housing Authority should have certain supervisory powers in regard to heating methods over housing schemes submitted by local authorities, and that the latter be empowered to make by-laws requiring the provision of smokeless heating arrangements in new buildings other than private dwelling-houses. This is probably as far as it is practicable to go in the present state of public opinion; for it seems wiser to rely upon its further education and enlightenment rather than upon vexatious legislative prohibition for the abatement of domestic smoke.

With regard to the question of industrial smoke, it was recommended by the Departmental Committee: (1) that the Ministry of Health should be given clearly defined power to compel or act in place of any defaulting authority which refuses to perform its duties in administering the law with regard to smoke; (2) that, instead of the present absolute prohibition, which it is impossible to observe, there should be imposed upon all manufacturers, users and occupiers of any business premises or processes, engines or plant of any description whatever, a general legal obligation to use the best practicable means, having regard to all the circumstances of the case, including the question of cost, for avoiding pollution of the air by smoke, grit, or any other noxious emissions; (3) that the same law should also apply to all Government establishments, and all rail and road locomotives and motor cars of whatever weight and type, and to steamers on rivers, estuaries, and lakes; (4) that the Ministry of Health should be empowered to fix smoke standards from time to time; (5) that the duty of enforcing the law with regard to the pollution of the air by smoke should be transferred from the local sanitary authorities, in whose jurisdiction it now rests, to the county authorities—*i.e.* the Councils of Counties and County Boroughs—albeit that minor authorities should still have the power to take proceedings, if they so desire; (6) that for proved breaches of the law much larger fines should be imposed than at present; and (7) that the Minister of Health should assign to one or more competent officers the duty of advising and assisting local authorities and manufacturers with regard to difficult smoke problems, and that such officers should report annually on the steps which are being taken, and the progress which has been made, in the suppression of avoidable smoke.

Since its inception in 1915, this Committee of the Association has carefully considered data and proposals relating to the causes and prevention of industrial smoke, and has been pleased to note progressive improvement, due largely to the increasing study of fuel economy in its various branches in regard to smoke abatement throughout the country, and that the Ministry of Health has taken evidence with a view to passing further legislation on more rational lines than that at present existing, which has become very much of a dead letter by reason of the extreme remedies it proposes.

It must be remembered that, even with the best appliances used with the greatest care, it is practically impossible altogether to prevent black smoke being produced for some small portion of the time when raw bituminous coal is burnt, and that in some industrial operations more smoke is apt to be produced than in others. It is evident, therefore, that no legislation should penalise the emission of what may be reasonably considered to be the minimum amount of smoke by competent judges who are familiar with the nature and the requirements of the particular industrial operation concerned. Indeed, it may be predicted that any attempt to do so would probably be as ineffective as the present enactments, and would thereby merely defeat its own purpose.

This Committee, having carefully considered the matter in its various aspects, has come to the conclusion that the best way of further abating the pollution of the atmosphere by smoke would be the institution by the Ministry of Health of a national Smoke Inspectorate on similar lines to the already existing Alkali Inspectorate, which has admittedly worked well and with beneficial results to the industry concerned.

Changes in Membership.—Since its last reappointment the following changes have taken place in the membership of the Committee. Mr. E. Bury retired on account of frequent absence abroad; Sir Joseph Walton, M.P., retired on account of ill-health, and Mr. Robert Armitage, M.P., was co-opted in his place. Mr. Wallace Thorneycroft has been co-opted in the place of the late Mr. G. Blake Walker, and Mr. E. Myers in the place of Mr. D. V.

Hollingworth. Also the following new members have been co-opted during the year, Messrs. J. E. Hackford, H. Stafford Rayner, L. L. Robinson, and W. C. P. Tapper.

The Committee recommends that it be reappointed to continue its investigations, with a grant of 5*l*.

APPENDIX.

Memorandum on Determining the Efficiency of Cooking Appliances.

The efficiency of appliances for the utilisation or transformation of heat energy cannot be regarded as so definite a function as in the case of mechanical or electrical ones. Since no non-conductor of heat is known, the actual thermal efficiency, however determined, must be variable according to the conditions.

In the case of cooking ranges and appliances, the conditions are so variable and complicated that it is difficult to devise test methods which shall invariably produce approximately the same numerical result from the same appliance on two or more separate occasions. Unless this is done, any figures must necessarily be uncertain, and no reliable comparison can be made between two different appliances in respect of efficiency.

In order that such figures should bear close relation to the efficiency of the appliance as used in practice, it is desirable that the experiments should be made under conditions similar to those in which the appliance is actually used, but unfortunately this is, in the nature of things, impossible. No reliable figures could be obtained while the apparatus was in use in cooking an actual article of food, because the amount of heat transmitted to such material cannot be measured, and also because of the extreme variations which arise in all the observable physical magnitudes during such an operation. Thus it is impossible to say when two similar joints are cooked to the same degree. It is, therefore, essential that the object heated for test purposes should be very different from an article of food, and such that the heat communicated to it should be capable of exact measurement.

Another difficulty in testing cooking ranges and the like is the question of 'residual heat.' The heat capacity of any cooking range is considerable; when put into operation the structure at first absorbs a large proportion of the heat generated, which remains after the useful operations are discontinued, and is only gradually dissipated thereafter. In making calculations from the results of a given test, this 'residual heat' cannot be ignored unless the cooking operations have been continuous and uniformly conducted throughout, but in ordinary practice the apparatus is rarely used in such a continuous manner. And as such 'residual heat' is usually much larger in amount than the total quantity utilised, it is evident that the efficiency cannot fail to be low in any practical case, and will vary greatly according to the weight of the appliance and the length and extent of the cooking operations.

The determination of any true efficiency can only be made by raising the apparatus to, and maintaining it in, a 'steady state' as nearly as possible, during which period valid observations on running efficiency can be made. It is, therefore, necessary to maintain the temperature as constant as possible over a long period in the steady state, and to integrate the entire results over a period of many hours.

The numerical results obtained during my experiments have in all cases been verified by the subsequent use of the same appliance in cooking a standard weighed menu, and there has been found to be a reasonably close correspondence between the fuel consumed during the practical operations and the figures experimentally determined.

The four principal sources of heat usable in cooking ranges are solid fuel, gas, electrical energy, and oil fuel. Fairly complete investigations have been made of solid fuel and gas, but the electrical series are in progress at the time of writing, and cannot be spoken of with the same degree of confidence as in the other two cases. The oil methods have not been investigated at all.

Of these four, solid fuel is by far the most difficult to handle in an experimental sense. It is impossible to measure the instantaneous rate of combustion. This cannot by any means hitherto discovered be maintained as constant as is

necessary for really accurate instantaneous observations, and to maintain a true 'steady state' in which the temperature of the range itself is kept accurately constant from hour to hour is not possible. Results can only be obtained by prolonging the experiment and obtaining a final result by a process of integration.

When a charge, however small, of fresh solid fuel is added to a glowing fire, the resulting combustion is essentially variable. When the rate of stoking is maintained artificially uniform, this variation of the reading takes the form of cyclical variations only approximately of sine character. Another considerable difficulty relates to the intensity of the draught. The same total rate of combustion can be produced in a variety of different ways by varying simultaneously the thickness of the fire and the intensity of the draught. Each such combustion results in a variation in the observed result.

The measure of the combustion is effected by analysing the flue gases. It was found impossible, with a range as ordinarily constructed, to obtain any such value of the CO_2 content of the flue gases as would correspond to really efficient operations with the ordinary commercial type of range. The general value showed that from eight to ten times the chemically necessary minimum of air was used. A high CO_2 value can only be secured by artificially suppressing the in-leakage of air. The value of the CO_2 content in any given appliance was found to be a correct measure of the efficiency.

With gas there are similar difficulties with regard to the surplus air, but not with regard to constancy of condition. With electricity there are no experimental difficulties, provided that the voltage of the supply is maintained constant.

Experimental Method.

Oven.—The interior temperature in an oven is a function whose significance is very small, for the following reasons : (1) the reading of the same thermometer in different parts of the oven varies widely (2) several thermometers of different type, all correct, and all, therefore, reading the same, when immersed, for instance, in hot oil, take up widely different readings when placed together in the same oven, essentially because the temperature of any object placed in any conditions is such that the net time rate of gain or loss of heat is zero.

Heat is lost or gained by any such object by radiation and convection. The relative influence of these two in any given environment depends on the size, shape, and radiativity of the surface of the bulb. Apart from any question of dimensions, no measurements of efficiency by means of thermometer readings would, therefore, in any case be possible. There is, in fact, no such function as the 'interior temperature,' and the expression 'mean interior temperature' is meaningless, except when its physical meaning is arbitrarily laid down. The only function to which this expression can reasonably be applied is the rate at which heat is transmitted to some extended object of conventional standard size, radiativity of surface and shape, placed in the oven, and of such size as to occupy a large part of the space. The expression 'efficiency' can only have a definite meaning when the object to which heat is communicated is precisely defined in these respects.

Exhaustive trials were made with vessels of standard shape filled with water, by observing the rate of rise of temperature, and calculating the transmission therefrom. This general method has the fatal disadvantage that great experimental difficulties are introduced, and with a large object in the oven having a rising temperature no sort of 'steady state' can possibly be maintained. The efficiency also varies considerably according to the temperature of the object to which heat is communicated.

A more convenient calorimeter is a coil of blackened copper pipe, through which a uniform stream of water is maintained. Special devices were designed and made for maintaining constancy in the water flow. The water may be at any temperature for which the efficiency is required. The flow is regulated so that the rise in temperature between in-flow and out-flow is not great. The coil as a whole can be maintained at a constant mean temperature. This method has the great experimental advantage that one reading of the difference in temperature between in-flow and out-flow suffices to give the current rate of

transmission, and the current variations in the thermal condition of the oven can, therefore, be determined.

The observed efficiency varies according to the temperature at which the coil is maintained. It is, therefore, necessary to lay down an arbitrary temperature at which all observations shall be made. If that temperature is high the resultant efficiency falls so low at low rates of combustion—even in extreme cases to zero—that its value cannot be accurately determined. As it is necessary to obtain a curve of efficiencies at all rates of combustion, it is, therefore, necessary to keep the mean temperature of the coil low, so that low rates of combustion may be included in the observations.

The temperature of the coil is of very great importance in the case of an ordinary gas oven. Where the temperature is low it enables the total heat of combustion to be brought into account; whereas when it is high the net heat only is available.

The use of gas enables much more accurate observations to be made, as a close approximation of the steady state is easily attainable by careful regulation of the rate of combustion. The latter can be controlled in the case of an oven so that any desired rate of transmission is maintained. In this case the principal difficulty is to determine how to deal with the difference between the gross and the net calorific value of the gas.

With a cold coil in the oven, a considerable part of the latent heat in the water vapour in the products of combustion may be communicated to the coil. The water appears as condensation on the surfaceof the coil, and drips down into the base. Thus obviously a portion of the latent heat is utilised. When the temperature is high there is no such condensation, and therefore no utilisation of this fraction of the heat of combustion. Whether and to what extent this latent heat is utilised in the actual cooking of an article of food it is impossible to say—probably some portion in the earlier stages, and later a re-evaporation of the same water originally condensed.

It will be evident that the efficiency as observed with this coil calorimeter will vary according to the surface area of the coil, or, what is the same thing, according to the size of the oven in which the coil is placed.

In order to compare, on a rational basis, ovens of different size it was necessary to investigate the question of the variation of apparent efficiency according to the size of coil used, and to specify the standard loading for an oven of any given size. It was found that when an oven is loaded to its fullest practical capacity the surface of all the food is approximately half the surface of the oven plates. This was, therefore, laid down as the standard loading.

The final efficiency therefore determined upon was the ratio between (1) the heat communicated to a coil whose exterior surface was half the interior surface of the oven when kept at a constant temperature, and (2) the amount of heat in the quantity of standard fuel burnt per hour. This function varies according to the rate of combustion, the efficiency increasing with increasing combustion. Its value was determined for three different rates, and a curve drawn through the three experimental points so obtained.

Hot Plate.—The efficiency in this case is the ratio of the maximum hourly amount of heat which can be communicated by the hot plate to ordinary cooking vessels to that in the fuel consumed per hour. This efficiency is fairly constant at all rates of combustion. The hot plate is covered with standard cooking vessels, each containing a weighed quantity of water, the rate of rise in which is determined.

Hot-water Supply.—This efficiency is the ratio between the heat communicated to the water in a weighed and measured cylinder and that in the fuel consumed. 20 lb. of fuel are used for this test.

Summary of Results.

The following table gives a summary of the usual values obtained by the methods outlined in the foregoing Appendix. It is to be understood that the phrase 'commercial' means a range or appliance as supplied by makers and used in an ordinary manner, and that the term 'specially designed' refers to appliances designed by me for these tests which, though expensive in construction, are thoroughly practical in character.

Solid Fuel Appliances.

		Found Efficiency Per cent.
Oven.		
Highest value with specially designed range		9·5
High values with commercial ranges	from	4·0 to 5·0
Usual values with commercial ranges	from	2·0 to 3·0
Lowest value with commercial range		0·8
Hot-Water Supply.		
Highest value with specially designed semi-independently fired boiler		42·0
High values with commercial appliances	from	15·0 to 20·0
Usual values with commercial appliances	from	8·0 to 15·0
Lowest value with commercial appliance		4·8
Hot Plate.		
Highest value with specially designed appliance		21·0
High values with commercial appliances	from	4·0 to 12·0
Usual values with commercial appliances	from	2·5 to 5·0
Lowest value with commercial appliance		1·14

Gas Appliances.

Oven.	
Highest value with specially designed oven	55·0
Highest value with commercial oven	38·0
Lowest value with commercial oven	17·0
Hot Plate.	
Highest value with specially designed appliance	60·0
Highest value with commercial appliance	55·0
Lowest value with commercial appliance (old pattern)	17·0
Griller alone (excluding boiling vessel standing over griller).	
Highest value with commercial appliance	19·0
Lowest value with commercial appliance	10·0

A. H. Barker

Absorption Spectra and Chemical Constitution of Organic Compounds.—*Report of Committee* (Professor I. M. Heilbron, *Chairman*; Professor E. C. C. Baly, *Secretary*; Professor A. W. Stewart). *Drawn up by the Secretary.*

In the report of the Committee presented in 1920 (*B.A. Report*, 1920, p. 222) the origin of the absorption bands exhibited by compounds was discussed, and a theory was put forward, based on the energy quantum theory, which gives an explanation of the phenomenon. Since 1920 a considerable amount of experimental work has been carried out which very materially supports this theory, and it becomes necessary to review the whole question of absorption spectra, because the situation has very materially changed during the last few years. There is no doubt that the original hypothesis as at first conceived of a direct correlation between the absorption bands shown by a substance in the visible or ultra-violet region of the spectrum and the constitution of that compound has been proved to be untenable. Since this hypothesis still seems to be accepted in its original form by some experimenters, it is advisable to state the reasons which militate against it.

The hypothesis had its origin in the work of Hartley and Dobbie on a few substances, such as isatin, carbostyril, and *o*-oxycarbanil, where it was found possible to determine the constitution of the parent substance by comparison of its absorption spectrum with those of its two methyl derivatives. Since it was found that the absorptive power of the substance agreed with that of only one of the two methyl derivatives, it was concluded that the constitution of the parent substance was the same as the constitution of that methyl derivative. From these observations there grew up a theory which postulated that there is a direct correlation between the primary structure of a compound and the absorption band it exhibits in the ultra-violet or visible region of the spectrum, and indeed that the absorption band can be taken as an index of its primary structure. Although there can be no doubt that there must exist some relation between the composition of a molecule and the frequencies of the light which it absorbs, it is certainly not a direct one in the above sense—that is to say, there is certainly no law which directly connects a given primary structure with a particular absorption band in that exceedingly minute section of the spectrum known as the visible and ultra-violet. The whole region of the spectrum, within which a substance is known to exert characteristic absorption, extends from $\lambda = 0.1\ \mu$ and $\lambda = 3.000\ \mu$, whilst the ultra-violet and visible region lies between $0.22\ \mu$ and $0.76\ \mu$, which is less than 1/5000th part of the whole. Apart from any other question, it seems somewhat arbitrary to base a fundamentally important conclusion on the observation of one absorption band in that tiny region, especially when it is remembered that that absorption band is only one of many exhibited by the substance throughout the whole spectrum. Then, again, there is the fact that a very large number of compounds do not exhibit any absorption band in that region, but only in the little known region between $\lambda = .22\ \mu$ and $\lambda = .1\ \mu$. This is ignored by the upholders of the absorption-constitution correlation, who only concern themselves with those particular substances which happen to exert absorption in the region between the wavelengths $0.76\ \mu$ and $0.22\ \mu$.

The arguments against this theory, which are based on actual observations, are overwhelmingly strong, and the more important of these may be given in detail. In the first place there are many substances which exhibit different absorption bands in the ultra-violet, according to whether they exist in the liquid or vapour state. Amongst the examples of this type of compound may be mentioned pyridine, piperidine, and benzaldehyde. If there existed a direct correlation between structure and absorption in the ultra-violet, it would be necessary to attribute two different structures to each of these compounds. Some recent results (Baly and Duncan, *Trans. Chem. Soc.*, **121**, 1008 (1922)) give strong evidence that the same is also true of ammonia, but the observation has not as yet been directly made, since the absorption bands of the gas and liquid phases of ammonia lie in the extreme ultra-violet region between $\lambda = 0.2\ \mu$ and

0.1μ. The difficulty of attributing two different structural formulæ to the molecules of pyridine and piperidine is sufficiently great, but in the case of ammonia such is impossible.

In the second place there may be mentioned the very remarkable changes in the absorption bands exhibited by a substance under different conditions of solvent, etc. A very striking example of such a substance is diphenylvioluric acid, which is white, whereas its alkali metal salts exhibit a progressive colour change from yellow to pure blue as the acid hydrogen atom is replaced by Li, Na, K, Rb, and Cs. It is inconceivable that each of these salts possesses a different structure in the way the theory demands. Then, again, there is the familiar case of the nitrophenols, which show a marked difference in absorption according to whether they exist in the free state or in the form of their alkali metal salts. At first sight this difference can be explained by attributing the quinonoid formula to the latter, and since the corresponding nitroanilines exhibit the same absorption bands these may also be given the quinonoid structure. Considerable difficulty, however, is then experienced with the nitrodimethylanilines, which exhibit the same absorption bands but are known not to be quinonoid. A further difficulty arises when the nitrophenols and their ethers are dissolved in concentrated sulphuric acid, when solutions are obtained similar in colour to the aqueous solutions of the alkali metal salts. These sulphuric acid solutions exhibit two or three absorption bands, and the theory would demand the co-existence of two or three structures differing from those of the parent substances and their alkali metal salts—a maximum of five different structures for one nitrophenol.

A very important phenomenon is exhibited by some members of the above class of substance. Anisole in alcoholic solution exhibits strong fluorescence, a fact that often escapes notice, since the emitted light lies in the ultra-violet region. In solution in strong sulphuric acid anisole exhibits a different absorption band from that shown by the alcoholic solution, and consequently the structure theory demands that anisole must possess a different structure in the two solvents. On the other hand, the frequency of the fluorescent light emitted by the alcoholic solution is the same as that absorbed by the sulphuric acid solution. This fact, which has been observed with a number of substances, strikes right at the root of the structure-absorption theory, the principal tenet of which is one structure one vibration frequency. One single substance, which possesses according to the theory two different structures, exhibits the same vibration frequency, and since one of these structures exhibits two frequencies, one of which is characteristic of the other structure, any argument directly connecting one frequency with one structure must necessarily fall to the ground.

These three sets of observations are sufficient to prove that the theory of a direct correlation between primary structure and absorption in the visible or ultra-violet region is untenable. As a matter of fact this theory is far too crude, in that it attempts to explain vibration frequencies in a very restricted region of the spectrum by purely arbitrary structures for which there is no positive evidence whatever. No attempt is made to find an explanation of the vibration frequency *per se*. No evidence, for instance, is given to support the view that a nitrophenol vibrates more slowly when it is quinonoid than when it is not. No explanation is offered of the fact that many substances of entirely different formulæ have almost identical absorption spectra. If there were any basis for the assumption of the absorption-structure relation, there surely would exist some definite connection between the various structural types, such as open chain, carbocyclic, and heterocyclic molecules, and the absorption spectra they exhibit. There is, however, no evidence for such a connection; indeed there is no order whatever in the observed phenomena from this point of view. Many compounds which are known to be similar in structure differ widely in their absorption spectra, whilst many compounds, the structure of which is as far different as can be imagined, exhibit very similar absorption spectra. There seems to be no rhyme or reason in the matter. The whole conception is based on the assumption that, because two isomers may exhibit different absorption, therefore different absorption must mean different structure. This assumption is still upheld by some, in spite of the overwhelming evidence against it and in spite of the total absence of any physical explanation of a single observation, because the upholders of this doctrine work in a water-

tight compartment, ignoring the absorption bands exhibited by their compounds in the spectral regions on either side of that which they arbitrarily select.

There are, however, certain observed facts in connection with absorption spectra which have now been established beyond all possible question, and these cannot be ignored in any discussion of the problem. In the first place, it may be categorically stated that every known substance, whether elementary or compound, possesses characteristic frequencies not only in the visible or ultra-violet, but also in the infra-red, and these frequencies, of course, are exhibited as absorption bands. In discussing these characteristic frequencies or absorption bands it is convenient to subdivide the whole spectrum into four sections, the visible and ultra-violet from $\lambda = 0.8\,\mu$ to $0.1\,\mu$, the short wave infra-red from $\lambda = 20\,\mu$ to $0.8\,\mu$, the long wave infra-red from $\lambda = 400\,\mu$ to $20\,\mu$, and the very long wave infra-red from $\lambda = 3{,}000\,\mu$ to $400\,\mu$.

The first real advance was made by Coblentz (*Carnegie Inst. Publ.*, No. 35, 1905), who examined the absorption spectra of a large number of substances in the short wave infra-red, and found that definite atomic groups, such as CH_3, OH, NH_2, etc., exhibit characteristic absorption bands in that region, of whatever compounds these groups may form a part. This is the first observation which definitely connotes constitution (not structure) with absorption.

The next advance was made by Bjerrum (*Nernst Festschrift*, p. 90, 1912), who applied Lord Rayleigh's principle to infra-red spectra and showed that the breadth of an absorption band is due to the summation of frequencies characteristic of a substance. If F be the characteristic frequency of a molecule in the short wave infra-red, then associated with F are the subsidiary frequencies $F \pm nR$, where R is a characteristic frequency of that molecule in the long wave infra-red and $n = 1, 2, 3$, etc. The absorptive power of these subsidiary frequencies decreases as the value of n increases, with the result that an absorption band is produced consisting of a series of equi-distant sub-groups which progressively decrease in intensity the farther they are from the centre. It was shown later (Baly, *Phil. Mag.*, **29**, 223 (1915)) that this structure is not confined to the infra-red bands, but is also shown by visible and ultra-violet bands.

A third phenomenon, which has considerable importance, is that the central frequency of every absorption band shown by a substance in the visible or ultra-violet is always an exact integral multiple of the central frequency of a very pronounced band shown by that compound in the infra-red (Baly, *Phil. Mag.*, **27**, 632 (1914)). Reference has been made to the fact that a given substance can exhibit different absorption bands in the visible and ultra-violet according to the conditions of solvent, etc., and it is these bands which form the basis of the structure-absorption theory. These bands, however, only differ in the fact that they are different integral multiples of the same infra-red band.

Further, the Bjerrum conception of the structure of absorption bands has been extended. It is well known that many bands have not only been resolved into sub-groups, but also that these sub-groups have been resolved into fine absorption lines. These absorption lines are symmetrically distributed in an analogous way to the sub-groups—that is to say, if S be the frequency of the principal line of a sub-group, the remaining lines in that sub-group are given by $S \pm nA$, where A is a constant and $n = 1, 2, 3$, etc. Just as in the case of the principal lines of the sub-groups, the frequencies of which are given by $F \pm nR$, where R is the frequency of an absorption line shown by the substance in the long wave infra-red, so the constant A is the frequency of an absorption line shown by the substance in the very long wave infra-red.

Lastly, the frequencies A, R, and F appear to be related together, as shown by the cases of sulphur dioxide (Baly and Garrett, *Phil. Mag.*, **31**, 512 (1916)) and water (Baly, *Phil. Mag.*, **39**, 566 (1920)). These two compounds are the simplest yet dealt with, and in both of them there are found evidences of the existence of three constants, A_1, A_2, and A_3, of the same order of magnitude, and of two larger constants, R_1 and R_2, these also being of like order of magnitude. The relation between these various frequencies is as follows: The frequency R_1 is the least common integral multiple of one pair of the three frequencies A_1, A_2, and A_3, whilst the frequency R_2 is the least common integral multiple of another pair of A_1, A_2, and A_3. Again, the frequency F, which is the central frequency

of the fundamental absorption band characteristic of the molecule in the short wave infra-red, is either the least common integral multiple of all three frequencies, A_1, A_2, and A_3, or a small multiple of that least common multiple. Finally, the central frequency of any absorption band shown by the substance in the visible or ultra-violet is an exact multiple of the frequency F.

The importance of these relationships becomes manifest when it is remembered that the frequencies R_1 and R_2 are those which Coblentz found to be characteristic of definite groups of atoms within the molecule. Another very important fact is that the absorption band shown by a molecule in the short wave infra-red, the central frequency of which has been denoted by F, is exhibited by that molecule under all conditions, whatever changes in the visible or ultra-violet absorption bands may be induced by change of solvent, etc. The conclusion, therefore, inevitably follows that the characteristic absorption band in the short wave infra-red is due to the molecule as a whole, that the long wave infra-red frequencies, R_1 and R_2, are due to groups of atoms forming part of the molecule, and that the frequencies in the very long wave infra-red, A_1, A_2, and A_3, are due to the atoms composing the molecule, whilst the visible and ultra-violet bands are dependent on the conditions under which the molecule exists.

There is no question but that the enunciation of the energy quantum theory by Planck marks a most important stage in the development of our knowledge of absorption spectra, and that this theory must necessarily govern all interpretation of absorption phenomena. Since all processes of absorption or emission of energy must be looked upon as being discontinuous and not continuous as previously supposed, it is very obvious that absorption spectra become placed on a quantitative basis. It was the vagueness of the old qualitative conception of absorption that permitted many theories to escape an early demise. The literature of the past twenty years abounds with such theories, which at best were only *ad hoc* proposals invented to explain isolated observations. The quantitative basis afforded by the conception of energy quanta shows up all the observations in true perspective, and enhances the value of the arithmetical relationships detailed above, for they can only be explained by energy quanta. One very simple example will illustrate this point, and the case of pyridine may be considered.

Pyridine in the liquid state exhibits an absorption band in the ultra-violet with a central wave-length of 258 $\mu\mu$ and a central frequency of 1.1628×10^{15}. When pyridine is absorbing light of this frequency the process is not continuous, but consists in the absorption of fixed amounts or quanta of energy, each quantum being the product of the frequency into the universal constant 6.57×10^{-27}. Each molecule of pyridine, therefore, absorbs a series of quanta of energy, each quantum being $1.1628 \times 10^{15} \times 6.57 \times 10^{-27}$ or 7.64×10^{-12}erg. Now let one molecule of pyridine absorb one quantum of 7.64×10^{-12} erg. This molecule will no longer be in equilibrium with its surroundings, and will, therefore, proceed to lose this energy in the form of heat—that is to say, the energy is radiated at an infra-red frequency. This radiation must also be emitted as quanta; and, further, it must be emitted as an exact whole number of quanta, since no energy can be destroyed. It follows that the one quantum absorbed must exactly equal the sum of the quanta radiated, and, therefore,

$$7.64 \times 10^{-12} = x \times F \times 6.57 \times 10^{-27}$$

where F is the radiating frequency. Hence we have

$$1.1628 \times 10^{15} = x \times F,$$

and therefore the ultra-violet frequency must be an integral multiple of an infra-red frequency. This, however, is known to be the case, for pyridine has an infra-red band, with central wave-length of 6.192 μ and a central frequency of 4.845×10^{13},

and $$1.1628 \times 10^{15} = 24 \times 4.845 \times 10^{13}.$$

The same will be true at whatever of its characteristic frequencies in the ultra-violet pyridine absorbs energy, all these being integral multiples of the infra-red frequency, 4.845×10^{13}.

This argument may be applied to any absorption band, and thus the central frequency of every absorption band shown by a substance must always be an

exact multiple of a smaller central frequency. This must not be interpreted to mean that the absorption bands characteristic of a substance extend to an unlimited distance in the infra-red. The frequencies in the very long wave infra-red, A_1 etc., are the smallest which characterise a molecule or its parts. This is proved by the fact that these frequencies exhibit themselves as single narrow absorption lines, and if there existed smaller frequencies still they would be associated with subsidiary frequencies due to their combination with those smaller frequencies in the manner already described. When a molecule absorbs one quantum of energy at one of its smallest frequencies, A_1 etc., it radiates this again as one quantum at that frequency.

This deduction from the Planck theory agrees, therefore, with the observed facts as regards the integral relations between the central frequencies characteristic of any one substance. The Planck theory, as it stands, however, does not offer an explanation of the second relation between the infra-red frequencies, namely, the principle of the least common integral multiple. There is no doubt that this principle must be connected in some way with energy quanta, and the question arises as to what this connection is. It has already been stated that Coblentz discovered the fact that the long wave infra-red frequencies, previously designated by R_1, are due to definite groups of atoms, and emphasis may be laid on the fact that a specific group of atoms exhibits its characteristic frequency whatever may be the composition of the rest of the molecule. This very clearly establishes the individuality of these frequencies and their origin in the relevant atomic groups. Then, again, more recent work shows that the same very long wave infra-red frequencies are common to two or more compounds which have the same atoms in common; and, further, these frequencies are combined in their least common integral multiple to give the frequencies of groups of those atoms. Since also the molecular frequency in the short wave infra-red is based on the least common integral multiple of all the very long wave infra-red frequencies present, the conclusion was drawn that these last are characteristic of the atoms themselves. If, as observation would prove, an atom is always characterised by the same frequency, of whatever molecule it forms a part, it must always absorb the same quantum of energy, and the natural deduction may be made that the atom is characterised by that quantum of energy.

This deduction that an atom is characterised by a definite quantity of energy gives a basis on which to formulate a theory of absorption which has certain material advantages over the Planck theory. The Planck theory starts from the assumption that molecules are characterised by certain vibration frequencies, and states that absorption and emission of energy at those frequencies are not continuous but discontinuous, and consists of the absorption or emission of energy quanta, each quantum being defined by the product of the frequency into the constant 6.57×10^{-27}. No attempt is made to explain why a molecule is characterised by specific frequencies, and for this reason the theory loses somewhat and lacks completeness. The theory also takes no account of the relationships between the various frequencies characteristic of a molecule, and hence becomes a theory of monochromatic absorption or radiation, in that these processes are assumed to be due to independent oscillators with a specific vibration frequency. Very important applications of this theory have, moreover, been made to the energy changes involved in chemical reaction, and in the great majority of cases the results experimentally obtained differ enormously from those calculated on this theory. The alternative theory, however, not only gives an explanation of these discrepancies, but also accounts for all absorption spectra observations.

This theory was fully given, up to the stage of development then reached, in the last report of the Committee (*B. A. Annual Reports*, 1920, p. 222), but in order that the recent evidence in its favour can be understood it is advisable briefly to re-state it. Four initial assumptions are made, which are quite straightforward and in each case the simplest possible. The first assumption is that every elementary atom is characterised by a definite quantity of energy associated with a definite physical process taking place within itself, such as the shift of one electron from one orbit to another. This quantity of energy is the same for all atoms of the same element, but is different for the atoms of

different elements. An atom can only absorb energy in terms of its fundamental unit or quantum, and as its energy quanta are absorbed the electron is progressively shifted from one orbit to another of greater radius. Conversely, the loss of energy by an atom consists in the shift of an electron from one orbit to another of smaller radius, each such shift being accompanied by the loss of the fixed quantity of energy, which may be called the atomic quantum of energy. These atomic quanta are of the order of from 6.5×10^{-16} to 2×10^{-15} erg.

The second assumption is that the electron shift occupies a definite period of time which is the same for all atoms. Since

$$\frac{\text{Quantum of energy}}{\text{Time factor}} = \text{Frequency}$$

it follows that an atom becomes endowed with the power of absorbing its energy quantum on exposure to radiant energy of a definite frequency. For the time factor the Planck constant, 6.57×10^{-27}, may be used, and therefore

$$\frac{\text{Atomic quantum}}{6.57 \times 10^{-27}} = \text{Atomic frequency.}$$

The characteristic frequencies exhibited by atoms are thus of the order of from 1×10^{11} to 3×10^{11}.

When atoms unite to form a molecule energy is lost in the process, and this energy can only have been given up by the atoms, for there is no other source from which the energy can have been derived. The case of two atoms entering into combination may first be considered. Both atoms lose energy when they combine, and the third assumption may be made that each atom loses the same amount of energy. This assumption is the simplest possible—namely, that each of the two atoms contributes one-half of the total energy lost when the two combine. It must, however, be remembered that each atom can only lose energy in terms of its atomic quantum, and for the sake of argument let the two atoms be characterised by the quanta 11×10^{-16} and 9×10^{-16} respectively. Since the two atoms can only lose their energy quanta, and since each of them loses the same amount of energy, it follows that the smallest amount each can lose is the least common integral multiple of the two atomic quanta. The first atom, therefore, will lose 9 quanta of the size 11×10^{-16} erg, whilst the second atom will lose 11 quanta of the size 9×10^{-16} erg. The total amount of energy lost in the combination will be $2 \times 11 \times 9 \times 10^{-16} = 1.98 \times 10^{-14}$ erg.

Before dealing with this principle of the least common integral multiple in greater detail a very important reservation must be made. The two quanta were assumed of 11×10^{-16} and 9×10^{-16}, but if, for example, the first quantum were 11.01×10^{-16} the least common integral multiple would be $1101 \times 9 \times 10^{-16}$. Indeed, the least common multiple of two numbers can have no possible physical significance unless the two numbers can be expressed in terms of a common unit (Campbell and Baly, *Phil. Mag.*, **41**, 707 (1921)). The fourth and last assumption may be made, therefore, that the atomic quanta of all atoms are integral multiples of a fundamental unit of energy. It may well be that this fundamental unit is the atomic quantum characteristic of the hydrogen atom, and there is much in favour of this being the case.

In the case of the molecule formed by the combination of two atoms having the atomic quanta of 11×10^{-16} and 9×10^{-16} erg respectively, the loss or gain of energy by this molecule may be considered. This molecule obviously can only lose or gain energy in terms of the atomic quanta characteristic of its atoms, and consequently the minimum amount of energy the molecule can gain or lose as a whole entity will be the minimum amount lost in its formation—that is to say, twice the least common integral multiple of the atomic quanta of its component atoms. This establishes the conception of a molecular quantum of energy, which, however, is not a physical entity in the same sense as an atomic quantum, but is the sum of an integral number of atomic quanta. Just as an atom is endowed with a frequency by virtue of its quantum, so also is a molecule endowed with a molecular frequency established by its quantum.

Although the two atoms have entered into combination, they cannot have lost their individuality as absorbers or radiators of energy. They still must possess the power of absorbing or evolving their characteristic quanta of energy.

Our freshly synthesised molecule, therefore, will be able to absorb or radiate its own molecular quantum and also the quanta characteristic of its two atoms. It will, on examination by absorption spectra methods, exhibit its molecular frequency together with the two atomic frequencies. Again, in making the first assumption that an elementary atom is characterised by a quantum of energy associated with the shift of an electron from one orbit to another, there is no need to restrict the atom to the possession of only one such electron. There may be several electrons of the same type, and as two or more of these may simultaneously undergo change of orbit, the atom will be capable of absorbing 1, 2, 3, etc. of its characteristic quanta at one time, and therefore will exhibit the corresponding frequencies. Our freshly synthesised molecule therefore will, on examination, be found to exhibit the molecular frequency of 3.0137×10^{12} and the atomic frequencies of $n \times 1.674 \times 10^{11}$ and $n \times 1.37 \times 10^{11}$, where $n=1, 2, 3$, etc. These correspond to the wave-lengths of $99.7\,\mu$, $\frac{1792\mu}{n}$, and $\frac{2190\mu}{n}$. This sets an upper limit to the two series of atomic frequencies, for, as will be seen, they converge at the 18th and 22nd terms respectively in the molecular frequency.

It follows from the foregoing that the energy lost in the combination of atoms depends on the size of their atomic quanta, and that these quanta have a very important significance. It may be noted in passing that the actual amount of energy evolved in the formation of the freshly synthesised molecule from the two atoms is small, and for one gram-molecule amounts to $1.98 \times 10^{-14} \times 6.23 \times 10^{23} \div 4.17 \times 10^{7}$, where 6.23×10^{23} is the Avogadro constant and 4.17×10^{7} is the mechanical equivalent of heat. The total energy, therefore, evolved in the formation of the freshly synthesised molecule when expressed as calories per gram-molecule is 296.

Although in the first assumption it was stated that an elementary atom is characterised by one energy quantum, there is no reason against an atom being characterised by two or more quanta of different sizes. It is probable that in some atoms there are two or more electrons, each of which requires a different amount of energy to shift it from one orbit to another. In view of the quantitative basis now given to the combination of atoms, it is very reasonable to suggest that the valency of an atom is the measure of the number of different atomic quanta associated with that atom. The natural corollary follows that the position of an element in the series of electropositivity is determined by the size of its largest atomic quantum.

The simplest case of the combination of two monovalent atoms was considered above, and it is now possible to deal with the next simplest case—namely, the synthesis of a molecule AB_2 from three atoms. Let the bivalent atom A be characterised by two quanta, 11×10^{-16} erg and 9×10^{-16} erg, and let the atom B be characterised by the quantum 7×10^{-16} erg. The first stage in the combination will be the formation of the molecule AB, with the establishment of the atomic group quantum of $2 \times 11 \times 7 \times 10^{-16}$ erg or 1.54×10^{-14} erg. The second stage will be the formation of the molecule AB_2 with the formation of the second atomic group quantum of $2 \times 9 \times 7 \times 10^{-16}$ erg or 1.26×10^{-14} erg. Both of these two atomic group quanta will be characteristic of the complete molecule, which can only gain or lose energy as a whole in terms of the true molecular quantum. This molecular quantum will naturally be twice the least common integral multiple of the two atomic group quanta—that is to say, $2 \times 1.386 \times 10^{-13}$ or 2.772×10^{-13} erg. This molecular quantum is four times the least common integral multiple of the three atomic quanta. This molecule differs from the simplest case of a binary molecule formed from two monovalent atoms in that it is characterised by two atomic group quanta as well as by the atomic quanta and the molecular quantum, and will be endowed with the corresponding frequencies. For the same reason as was given above for atomic frequencies there will also be series of atomic group frequencies, and therefore the total number of frequencies exhibited by this molecule will be as follows: three series of atomic frequencies, $n \times 1.674 \times 10^{11}$, $n \times 1.37 \times 10^{11}$, and $n \times 1.0655 \times 10^{11}$, where $n=1, 2, 3$, etc.; two series of atomic group frequencies, $n \times 2.344 \times 10^{12}$ and $n \times 1.918 \times 10^{12}$; one true molecular frequency, 4.22×10^{13}. The first and

third atomic frequency series will converge at the frequency 2.344×10^{12}, whilst the second and third will converge at the frequency 1.918×10^{12}. These atomic group or intra-molecular frequencies form the first members of two new series which converge at the molecular frequency 4.22×10^{13}. It may be noted that this molecular frequency lies in the short-wave infra-red and corresponds to the wave-length of 7.1μ.

These relationships are exactly those which have been observed. The complete structure of the absorption bands of SO_2 and H_2O, which is that described above, was detailed in the last report of the Committee, together with the subsidiary frequencies which are associated with the molecular and intra-molecular frequencies to give broad absorption bands. No further discussion, therefore, is necessary.

The two most important facts which must be emphasised are, first, the characterisation of a molecule by its own molecular quantum, and, second, the possibility of a molecule gaining an amount of energy equal to its characteristic quantum on exposure to radiation of frequencies equal to those of its atoms or component groups of atoms. The molecular quantum is the most fundamentally important characteristic, since it not only determines the absorptive power of a molecule in the visible and ultra violet, but also forms the quantitative basis of every chemical reaction which the molecule undergoes. The importance of the molecular quantum will be understood from a consideration of the affinity which causes atoms and molecules to react together. The first assumption, which forms the basis of the present theory, states that the atomic quantum is the amount of energy required to shift an electron from one orbit to another, the electrons being conceived as normally rotating in fixed orbits round the central positive nucleus of the atom. This rotation of a negatively charged particle will establish an electro-magnetic field, the force lines of which pass through the plane of rotation. An atom, therefore, forms the centre of a force field and will possess two faces, which may be called positive and negative. If two atoms approach one another in such a way that their like faces come together, they will repel one another, but if their unlike faces come together they will attract one another. When this happens, energy will be lost by the two atoms in the manner already described—that is to say, each atom will lose an amount of energy equal to the least common integral multiple of the quanta of the two atoms. In common parlance the two atoms enter into chemical combination, and in order to decompose the freshly synthesised molecule an amount of energy will be required which is exactly equal to that lost—namely, one molecular quantum. The combination of the two atoms was due to the attraction between one face of each, and consequently there are yet to be considered the two remaining faces of the atoms in the freshly synthesised molecule. It is impossible that the force lines at these two faces, which are of opposite type, can exist without mutual influence, and condensation must ensue between these with the further escape of energy. A freshly synthesised molecule, therefore, is metastable, since the external force fields of its atoms must condense together to form a molecular force field. The loss of energy involved in this condensation is a process in which the molecule as a whole takes part, and consequently the energy can only be lost in terms of the molecular quantum. A freshly synthesised molecule must thus pass into one of a number of possible phases, each consecutive phase differing in energy content by one molecular quantum and also in the condition of its molecular force field. It is evident that in this process the molecule will not suffer any loss of individuality as far as its characteristic quanta are concerned. None of the deductions made above will be contradicted, and the only change will be the endowment of the system with an additional quantum, and therefore an additional frequency.

Let the ternary molecule discussed above be again considered. If this molecule when in the freshly synthesised state absorb one molecular quantum of 2.772×10^{-13} erg, it will just be resolved into its component atoms. If now, after the formation of its molecular force field with the loss of one molecular quantum, it is wished to resolve the molecule into its atoms, two quanta or 5.544×10^{-13} erg will be necessary. The molecule and its force field can absorb these two quanta simultaneously, and therefore the system becomes endowed with a new quantum which is twice the molecular quantum. In general, if the molecule, during the formation of its force field, lose $x \times 2.772 \times 10^{-13}$ erg, the

resulting molecular phase will be endowed with the quantum $(x+1)\times 2.772\times 10^{-13}$ erg. In addition, therefore, to the atomic quanta, intra-molecular quanta, and molecular quantum, a molecule will be characterised under normal conditions by a phase quantum which is an integral multiple of its molecular quantum. It will consequently exhibit a phase frequency which is an integral multiple of the molecular frequency. It is a fact well established by observation that the phase frequency is always situated in the visible or ultra-violet region of the spectrum, and indeed the absorption bands on which the old structure-absorption theory was based are those due to the molecular phases. If the above molecule lost in the formation of its force field 14, 18, 22, 26, 30, or 34 molecular quanta, the phase produced would be characterised by an absorption band with central wave-length of 474, 374, 309, 263, 229, or 203 $\mu\mu$ respectively. These molecular phases may be considered in detail from the point of view of absorption spectra in the visible and ultra-violet, the physical properties, and the chemical reactions of molecules.

The particular phase into which a freshly synthesised molecule will pass when in the free state depends on two factors, the relation between the external force fields of its atoms and the temperature. If the external atomic force fields happen to be equally balanced—that is to say, if the positive and negative affinities of these are equal and opposite—the condensation between them will proceed very far, with the loss of a great number of molecular quanta. In this case the phase frequency will be situated in the extreme ultra-violet which lies beyond the working limit of a quartz spectrograph in air, and it has been customary to record such a substance as being diactinic. This, however, is utterly misleading, for it is now well known that every substance exhibits selective absorption in some part of the spectrum with shorter wave-length than 800 $\mu\mu$, and a statement that any substance is diactinic is incorrect. If, on the other hand, the external atomic force fields are not equally balanced, the molecular force field condensation will not proceed so far, and the phase frequency will then lie in the visible or near ultra-violet region and will be observed with an ordinary spectrograph.

When a large number of molecules are present it by no means follows that they all exist in one phase, and observation shows that this is not the case, but that two or more phases exist in equilibrium with one another, the equilibrium for a given compound depending on the temperature. Even when a gas or liquid shows an absorption band in the visible or near ultra-violet, the proportion of the molecules which exist in the phase with frequency in that region is small, the great majority existing in a more condensed phase. Although but little work has as yet been carried out on direct observations of absorption bands in the extreme ultra-violet by means of the vacuum spectrograph, the existence of these bands can be proved and their frequencies calculated from measurements of the refractivity. It has been shown (C. and M. Cuthbertson, *Phil. Trans.*, 213, A, 1 (1913)) that the refractivities of the simple gases can be expressed by a modified Sellmeyer formula :

$$n-1=\frac{N}{v_0^2-v^2},$$

where v_0 is the central frequency of the absorption band in the extreme ultra-violet, N is a constant, and v is the frequency for which the refractive index has the value n. This formula can readily be extended to apply to substances the molecules of which exist in two or more phases in equilibrium. When two phases are present, if v_1 and v_2 are the frequencies of the two phases, and V_1 and V_2 are the volumes occupied by the two phases, then

$$(n-1)(V_1+V_2)=\frac{N}{v_1^2-v^2}+\frac{N}{v_2^2-v^2}.$$

This formula very accurately expresses the refractivities of a substance which exhibits one absorption band in the near ultra-violet, and it is therefore established that the substance also exhibits an absorption band in the extreme ultra-violet. It is interesting to note that the number of molecules in the less condensed phase is very small, being of the order of 0.2 per cent. of the whole.

As regards the effect of temperature on the phase in which molecules exist, it must be remembered that the only method by which a molecule can absorb or evolve energy is in terms of its atomic quanta, or of integral multiples of these. Whatever may be the phases co-existing, increase of temperature means the supply of whole numbers of atomic quanta, and therefore an inevitable change of phase in some of the molecules. This is evidenced by the change in refractivity observed with change in temperature, and it has been found possible in this way to express the specific heat of a compound in terms of its molecular quantum. Since the frequencies of all the absorption bands shown by a substance in the visible and ultra-violet are exact multiples of the molecular frequency, the calculation of the constants in the Sellmeyer formula is very simple. Let a substance having a molecular frequency of v_x exist as an equilibrium mixture of two phases, the phase frequencies of which are xv_x and yv_x respectively, x and y being two whole numbers. Further, let the frequency xv_x lie in the near ultra-violet and therefore be known, and let the volume of the molecules existing in this phase be 1 and the volume of those existing in the phase with frequency yv_x be V. The formula then becomes

$$(n-1)(V+1)=\frac{N}{xv_x{}^2-v^2}+\frac{NV}{yv_x{}^2-v^2},$$

and since xv_x is known the values of N, V, and yv_x can readily be found from four measurements of the refractive index. When the temperature of the substance is raised, the amount of energy absorbed can readily be calculated from the molecular specific heat and the rise in temperature, and from this the number of molecular quanta that have been absorbed can be found. Since the absorption of one molecular quantum changes one molecule from one phase into the next, the total number of molecules that have changed phase can be determined.

This method of calculation has been proved to be correct in the case of methyl hexyl ketone, the refractive indices of which have been determined at 10° and 80°. It happens that the amount of energy necessary to raise the temperature of one gram-molecule of the ketone from 10° to 80° is very nearly equal to two molecular quanta per molecule, and, therefore, very nearly the whole of the molecules undergo a change from their original phase into the next phase but one. The ketone exhibits an absorption band in the near ultra-violet, and, since this band does not change when the substance is warmed, the phase change is restricted to the molecules which exist in the phase with frequency in the extreme ultra-violet. As the number of molecules in this phase is relatively very large, it will be sufficiently accurate to assume that all these undergo the two changes of phase.

At 10° the refractivity of methyl hexyl ketone is very accurately expressed by the general formula

$$(n-1)(V+1)=\frac{N}{xv_x{}^2-v^2}+\frac{NV}{yv_x{}^2-v^2},$$

and at 80° the refractivity (duly corrected for the change in density) is expressed by the formula

$$(n-1)(V+1)=\frac{N^1}{xv_x{}^2-v^2}+\frac{N^1V}{(y-2)v_x2-v^2},$$

the accuracy being well within the experimental error. This phase change produced by temperature change has also been proved with other substances, and the phenomenon is one of great importance. It not only affords conclusive proof of the existence of a large proportion of the molecules in a phase with frequency in the extreme ultra-violet, even when the compound shows an absorption band in the visible or near ultra-violet, but it also proves that the specific heat of a substance can be quantitatively expressed in terms of the molecular quantum of that substance. It may be noted in passing that, whilst it is a general rule that the greater proportion of the molecules of a substance exist in a phase with frequency in the extreme ultra-violet, there are some molecules the greater proportion of which exist in a phase with frequency in the visible region. This is the case with the dyestuffs, the extraordinary intensity of colour

of which is due to the fact that the majority of their molecules exist in the phase with visible colour.

Analogous to the specific heat relation is the expression of the latent heat of evaporation of a compound in terms of its molecular quantum. That this is possible can readily be seen from the following examples (Baly, *Phil. Mag.*, **40**, 15 (1920)).

—	Observed molecular latent heat	Latent heat in ergs per mol.	Molecular quantum	Number of quanta	Calculated molecular latent heat
Carbon disulphide .	6384	$4{\cdot}332\times10^{-13}$	$4{\cdot}278\times10^{-13}$	1	6305
Water . . .	9650	$6{\cdot}548\times10^{-13}$	$3{\cdot}218\times10^{-13}$	2	9485
Ammonia . .	4950	$3{\cdot}359\times10^{-13}$	$1{\cdot}64\ \times10^{-13}$	2	4834
Acetic acid . .	5094	$3{\cdot}458\times10^{-13}$	$1{\cdot}711\times10^{-13}$	2	5044
Methyl acetate .	8166	$5{\cdot}541\times10^{-13}$	$2{\cdot}772\times10^{-13}$	2	8170
Ether . . .	6660	$4{\cdot}519\times10^{-13}$	$2{\cdot}249\times10^{-13}$	2	6629
Chloroform . .	6984	$4{\cdot}739\times10^{-13}$	$2{\cdot}371\times10^{-13}$	2	6988
Carbon tetrachloride	7190	$4{\cdot}879\times10^{-13}$	$1{\cdot}526\times10^{-13}$	3	6744
Sulphur . .	23160	$1{\cdot}572\times10^{-12}$	$1{\cdot}671\times10^{-13}$	9	22158

In most cases the change in phase on vaporisation is restricted to those molecules in the equilibrium the frequencies of which lie in the extreme ultra-violet, and, therefore, no difference is found between the absorption spectra of the liquid and vapour in the visible or near ultra-violet. Sometimes, however, on vaporisation there is also a phase change on the part of those molecules present, the phase frequency of which lies in the visible or near ultra-violet. Mention has already been made of these substances, the best known examples being pyridine, piperidine, and benzaldehyde.

In addition to the effect of temperature—that is to say, very long wave radiations—in causing a phase change of molecules any method of supplying energy to the molecules will be equally effective. There are two other methods of supplying energy to substances—namely, by exposing them to short-wave radiation of frequency equal to their phase frequencies, and also by the action of a solvent or catalyst. These two methods may be discussed separately, and the effect of short-wave radiation may be dealt with first. It was stated in the early part of this report that the molecular quantum is exactly equal to the amount of energy given out when the component atoms combined to form that molecule. A freshly synthesised molecule, therefore, on absorbing one molecular quantum will just be resolved into its atoms. After the molecular force field has been established with the evolution of a number of molecular quanta, the absorption of one molecular quantum will simply change the phase into the next phase of higher energy content. It has already been shown that, if during its force field condensation a molecule loses, say, 16 molecular quanta, the resulting phase will be characterised by a quantum which is 17 times the molecular quantum, and consequently when this molecule is exposed to radiation of a frequency equal to its phase frequency, it will absorb a quantum which is 17 times its molecular quantum. This molecule, however, was synthesised with the loss of one molecular quantum, and has subsequently lost a further amount of 16 molecular quanta, or 17 molecular quanta in all. The absorption of one phase quantum, therefore, is always sufficient just to resolve a molecule into its atoms. It does not follow that a molecule must be decomposed into its atoms when it absorbs a phase quantum, for observation shows that in general the molecule is thereby converted into a phase of higher energy contact, the balance of energy being radiated to the surroundings. Let a freshly synthesised molecule, for example, lose 19 molecular quanta in its force field formation and then absorb its phase quantum, which is equal to 20 molecular quanta. In general, the molecule is converted into another phase, and let this new phase be that characterised by a phase quantum of 17 molecular quanta. Clearly, three molecular quanta are required for this phase change, and, therefore, the balance

of 17 molecular quanta is radiated to the surroundings. This excess energy may be radiated either as 17 quanta at the molecular frequency or as one quantum characteristic of the new phase. This second alternative, which is of frequent occurrence, is the familiar phenomenon of fluorescence, and it is important to note that the frequency of the fluorescence is always characteristic of the molecular phase into which the molecules are changed by the exciting light. To be strictly accurate, it should be stated that fluorescence always occurs when a molecule is exposed to light of its phase frequency, but when the emission of energy takes place in the infra-red this energy merely evidences itself as heat.

The phenomenon of phosphorescence is exactly analogous, but in this case a reservation must be made. In phosphorescence the emission of energy persists after the exciting light has been removed, and consequently the phosphorescence must be the energy evolved when the newly produced phase reverts to the normal phase. It follows from this that the frequency of phosphorescent emission can only be a sub-multiple of the activating frequency, as otherwise the phosphorescence frequency cannot be characteristic of the newly produced phase. Thus, if a phase quantum of 20 molecular quanta is absorbed and the phase is produced which is characterised by the quantum of 10 molecular quanta, then on reversion to the original phase this new phase can emit its own quantum of 10 molecular quanta. Again, if the newly produced phase is characterised by a quantum of five molecular quanta, it can emit during reversion to its original phase three of its characteristic quanta. An important confirmation of this explanation of phosphorescence is to be found in the well-known fact that an activated substance very rapidly loses its energy, as phosphorescence when exposed to infra-red radiation. The activated substance is in a metastable condition, and this condition is disturbed by the absorption of infra-red quanta, with the result that the system rapidly reverts to its normal phase equilibrium.

The change in phase of molecules on exposure to radiation of their phase frequency can readily be observed by absorption spectra methods, but, since these are only applicable to gases and liquids, it is necessary that the newly produced phase be stabilised in some way, as it otherwise reverts instantly to the original phase. A typical example is afforded by an alcoholic solution of trinitrobenzene, containing a trace of piperidine. On exposure to ultra-violet light of the phase frequency of trinitrobenzene the solution turns red, owing to the conversion of the trinitrobenzene into a phase of greater energy content. On remaining in the dark this new phase reverts to the original phase, and the solution again becomes colourless. The phase change of solid substances on exposure to light of their phase frequency is a familiar phenomenon as exemplified by photography, and by the change in colour of many inorganic salts on exposure to rays of very short wave-length.

Up to the present the phases in which molecules exist have only been considered for substances in the free state, when the phase equilibrium is determined only by the relation between the atomic force fields and by the temperature. There yet remains to be discussed the influence of a solvent which is one of great importance, because a solvent not only can alter the equilibrium between the phases in which a substance normally exists, but also can cause the molecules to pass into new phases. From the point of view of ordinary absorption spectra observations the latter property of a solvent is of greater interest than the influence of radiant energy in the form of heat or light, since it is these phase changes which for so long have been subpœnaed as evidence in favour of the structure-absorption theories. The explanation of the action of a solvent follows very readily from what was stated above about the formation of the molecular force field. When the atomic force fields of a freshly synthesised molecule are unequally balanced, the condensation between them with the evolution of molecular quanta to form the molecular force field cannot proceed very far, since sooner or later a balance of one or other type of affinity will remain uncompensated, and this will arrest the condensation process. If this uncompensated balance be removed in some way there will be nothing to prevent the force field condensation from proceeding further with the evolution of more molecular quanta. The extent to which this proceeds will depend on the atomic fields themselves. Since the uncompensated balance of affinity endows

the molecule with the power of forming addition compounds with other molecules, it forms the basis of what is recognised by chemists as residual affinity. Owing to the existence of two types of affinity with their origin in the two faces of the atoms, there will be two types of residual affinity which evidence themselves as acid and basic respectively. If two molecules which possess basic and acid residual affinity respectively come together they will tend to form a complex molecule, and this complex can be stabilised in one of two ways, depending on the relation between the atomic fields of the two component molecules.

In the first place the complex may lose energy as a whole to the surroundings, with the result that it cannot be dissociated until this amount of energy has been supplied to it. In the formation of this type of complex both components pass into a more condensed phase, and it forms the basis of salt formation, such as aniline hydrochloride, etc.

In the second place, if the tendency of the atomic fields to undergo condensation is greater in one molecule than it is in the other, then, when the complex is formed, the first component molecule will give up one or more molecular quanta to the second component molecule. Again will the complex molecule be stabilised, since, although no energy has been evolved to the surroundings during its formation, the complex cannot be dissociated unless the second component molecule render up to the first the one or more molecular quanta. It is, of course, necessary, in order that such transference of molecular quanta can take place, that the two molecules be characterised by molecular quanta of exactly the same size. This identity is, however, secured, because it has been proved by absorption spectra observations in the short wave infra-red that, when a complex of two or more molecules is formed, it is characterised by its own single molecular quantum. The conditions, therefore, for the transference of energy from one component to the other are perfect (Baly and Tryhorn, *Phil. Mag.* **31**, 417 (1916)).

It is evident from this that both components, in forming such a complex, must undergo a change in phase, since the first component which gives up one or more molecular quanta must pass into a more condensed phase, whilst the second component which accepts these quanta must pass into a less condensed phase. This explanation, therefore, is capable of experimental proof, since the frequency of the first molecule in the free state must shift towards the ultra-violet, whilst the frequency of the second molecule in the free state must shift towards the red. The absorption spectra of several of these complexes have been observed, and, without entering into the experimental details, it may be stated that in each case the change in frequency in opposite directions on the part of the two molecules in forming the complex has been established. Complexes of this type are familiar to the organic chemist, and the type may be exemplified by those formed by picric acid with the aromatic hydrocarbons, diphenylamine with chlorobenzaldehyde, etc.

The effect of a solvent in changing the phase of a substance may, therefore, be readily understood, since it is the solvent molecules which supply energy to the solute molecules. Indeed, this energy transference is the essential characteristic of the phenomenon of solution. Moreover, the existence of the same compound in different phases when in solution in different solvents is also explained, the particular phase formed depending on the number of molecular quanta supplied to each solute molecule by the solvent molecules. There are many instances known of this phenomenon, which is one of the commonest observed in the study of the absorption spectra of organic compounds. One of the most striking cases is that of trinitrobenzene, which exists in four different molecular phases when in solution in solvents of different degrees of basicity. Other examples were given in an early part of this report when the structure-absorption theory was criticised. The real explanation of these changes in absorption, and in some cases of changes in visible colour, observed with different solvents is now to hand, because they are due to the formation of different phases of one and the same molecule by the supply to it of one or more molecular quanta by the solvent. It cannot be denied that in certain specific instances, such as the nitrophenols, it is possible to write a different structure when a molecule exhibits different absorption bands, but these instances are very few in number compared with those in which structural changes cannot be written,

There is, moreover, not one tittle of evidence to support this structural juggling with molecules, and the whole conception, as previously stated, is based on the mistaken notion that a molecule must change its structure because it changes its absorption frequency in the visible or ultra-violet and nowhere else in the spectrum.

One of the greatest protagonists of this theory is Hantzsch, whose brilliant experimental work in support of his views was described in the last report of the Committee. It is of some interest to note that the foundation of his work lies in acetoacetic ester and its two ethyl derivatives, ethyl diethylaceto-acetate, and ethyl β-ethoxycrotonate. Since acetoacetic ester in alkaline solution exhibits an absorption different from that shown by each of its ethyl derivatives, Hantzsch said it must have a different structure, and on this basis he built up his theory. On the other hand, there is to be recorded the damning fact that ethyl diethylacetoacetate in alkaline solution exhibits the same absorption as does the parent ester in the same solvent. Hantzsch countered this observation by stating that the diethyl compound must have contained some of the monoethyl compound. Some recent work, however, with absolutely pure ethyl diethylacetoacetate has entirely confirmed the original observation, which strikes at the root of the whole of Hantzsch's work. These investigations, which have been accurately carried out with more modern apparatus than Hantzsch had at his disposal, have also proved that none of the absorption phenomena exhibited by these compounds can possibly be explained by any structural theory.

Another interesting example of this phenomenon, described by Hantzsch and already mentioned, is diphenylvioluric acid, which forms a series of differently coloured salts with the alkali metals, Li, Na, K, Rb, and Cs. Since these salts show different absorption bands Hantzsch suggested different structures for them, but they are only different phases of one molecule, established by the supply of one or more molecular quanta by the alkali hydroxide. In order to avoid misconception, it must be stated that the complex first formed is $C_{16}H_{13}O_4N_3$, MOH, and the fact that water is subsequently lost does not alter what has been said previously. If any doubt be felt as to the correctness of the explanation by the formation of different phases, this must surely vanish when it is remembered that the various frequencies exhibited by these salts are integral multiples of the molecular frequency of the diphenylvioluric acid molecule, since they show constant frequency differences amongst themselves.

It might be considered that the theory of molecular phases is incomplete without any evidence of the free existence of a compound in phases different from those in which it normally exists. To such criticism the reply might be made that the three physical states of a compound—solid, liquid, and gas—are due to its existence in different phase equilibria, this being proved by the quantitative basis that was established above on the phase theory of specific heat and latent heat. This, however, might not be considered to be sufficiently convincing, because the change in phase which accompanies the change in state has not, except in a few instances, been directly proved by absorption spectra observations, since as a general rule the phase change concerns those molecules which exist in phases characterised by frequencies in the extreme ultra-violet. On the other hand many cases are known of compounds which have been obtained in phases of higher energy content than their normal phases, and indeed these metastable phases are often visibly coloured, whilst the normal and stable phases are white or only faintly yellow. A typical instance is trinitrotoluene, the normal colour of which is slightly yellow. In solution in piperidine, trinitrotoluene exists partly in a phase characterised by a red colour, and if this solution is poured into excess of strong hydrochloric acid and the resulting brownish-red solid is dissolved in benzene and the solution filtered, the metastable red-coloured phase, which is insoluble in benzene, is obtained as an oil. This substance is pure trinitrotoluene, and shows the same absorption spectrum as the original piperidine solution. On treatment with nitric acid it at once reverts to the normal and stable form.

Another instance is afforded by the so-called *aci*-nitrophenol ethers, obtained by Hantzsch and Gorke, who treated the silver salts of the nitrophenols with alkyl iodides at very low temperatures. These substances were considered to

have a quinonoid structure owing to their visible colour, but there is no evidence whatever to support this conclusion. The extraordinary readiness with which they pass with evolution of energy into their stable colourless phases strongly supports their being metastable phases having the usually accepted structure. Other examples of this phenomenon may be found in the existence of many substances in two modifications, of supercooled liquids, etc., but as yet the phase explanation of these lacks the corroborative evidence of absorption spectra observations.

Although these molecular phases have been discussed for compound molecules, they also exist in the case of elementary molecules, and there is little doubt that the phenomenon of allotropy is merely one of equilibria between different phases of the same molecule. The allotropy of sulphur has been examined from this point of view, the various allotropic modifications being different equilibrium mixtures of the four phases $S\lambda$, S-, $S\phi$, and $S\mu$. Each of these phases in solution in the proper solvent—namely, carbon bisulphide, toluene, chloroform, and piperidine, respectively—exhibits its own absorption band, and the central frequencies of these bands are all exact multiples of the molecular frequency of sulphur in the infra-red. This explanation of allotropy has also been given by Smits, whose molecular species are in reality molecular phases.

Two final examples may be given of this phenomenon, and the first is the preparation of two kinds of ammonia which differ in the equilibrium between the two phases present. These two types of this gas can be obtained by the slow and explosive evaporation of the liquefied ammonia, and they differ very remarkably in the amount of energy required to decompose them into nitrogen and hydrogen (Baly and Duncan, *Trans. Chem. Soc.*, **121,** 1008 (1922)). The gas obtained by the sudden evaporation of the liquid contains more than the normal proportion of the more condensed phase, and therefore requires more energy to decompose it.

The last example is found in the most interesting results obtained by Baker (*Trans. Chem. Soc.*, **211,** 568 (1922)) on completely drying a number of liquids. He found that after drying them over phosphoric oxide for a number of years the boiling-points of the liquids were very considerably raised. There is no doubt that the normal phase equilibrium in these liquids at ordinary temperatures is maintained by the traces of water present. On complete removal of the water the phase equilibrium is shifted towards the side of the more condensed phase. In order to convert the dried liquid, therefore, into the normal vapour, more energy will be required than with the moist liquid, and thus the boiling-point will be raised.

In concluding this section of the report, the phase theory may be applied to one of the many interesting problems of the organic chemistry of to-day—namely, that of free radicles. It is known that compounds of the type of hexaphenylethane, $(C_6H_5)_3C-C(_6H_5)_3$, dissociate into the free radicles, *e.g.* triphenylmethyl, $(C_6H_5)_3C$, and considerable difficulty is found in explaining why less energy is required to dissociate such a compound than is required in the case of the parent substance, ethane, the bond between the two central carbon atoms being the same in the two cases. The explanation, however, is at once supplied by the phase hypothesis and supported by absorption spectra observations. In the case of ethane the external force fields of the atoms are well balanced, so that the molecular force-field condensation proceeds far with the evolution of many molecular quanta and the formation of a phase of small energy content. This is proved by the fact that the phase frequency of ethane lies in the extreme ultra-violet. In order, therefore, to dissociate a molecule of ethane into free methyl radicles the amount of energy required will be very large indeed. When the hydrogen atoms of ethane are progressively substituted by phenyl, the balance between the atomic force fields becomes more and more disturbed, and in hexaphenylethane this effect is so pronounced that the molecular force-field condensation cannot proceed very far. A much smaller number of molecular quanta are lost, and the frequency of the resulting phase lies on the border of the visible region. A much smaller amount of energy will thus be necessary to dissociate this compound, owing to the fact that it exists in a phase of much higher energy content. The difference between the two compounds, ethane and hexaphenylethane, as regards their dissociation

is thus entirely due to the different phases they exist in, and has nothing to do with the bond between the two central carbon atoms, which is the same in both compounds. If the two compounds existed in the same phase, the same amount of energy would dissociate them both.

Part II.

In this section a brief account may be given of the application of molecular phases to chemical reactions, for it can be shown that their existence receives thereby very strong support. Although, perhaps, the connection between this application and absorption spectra might at first sight appear to be only indirect, yet it is self-evident that if the proof of the existence of molecular phases depended on absorption-spectra observations alone the arguments in their favour would be less convincing. Since the existence of these phases is founded on the reactivity of elementary atoms expressed on a quantitative basis, their failure to give a quantitative basis to the reactions of molecules would militate strongly against their reality. It must be admitted, even by the most sceptical, that any corroborative evidence in their favour, even if such evidence have no immediately apparent bearing on absorption spectra, must materially strengthen those arguments which were based on absorption spectra alone.

As was explained in the preceding section, the affinity of atoms for one another, which causes them to combine with the loss of whole numbers of their atomic quanta of energy, is due to the electro-magnetic fields of the atoms. The resulting freshly synthesised molecule is in a metastable state, since each of its component atoms possesses an external force field, and condensation between these fields must ensue with the loss of a definite number of molecular quanta and the formation of a molecular phase, each possible phase being defined by the number of molecular quanta that have been lost in its formation. It follows from this that the condition of the molecular force field must be different in each phase of a given molecule, and, since the reactivity of a molecule must depend on its affinity and power of attracting other molecules, the reactivity of each phase of a given molecule must be different. There is a great mass of evidence which establishes this difference in reactivity between the various phases of a given molecule, and which will be discussed in this section. A typical instance may be mentioned here—namely, that of benzaldehyde—which exhibits entirely different absorption spectra when in solution in alcohol and in strong sulphuric acid, and therefore exists in different phases in these two solvents. When in alcoholic solution benzaldehyde is readily oxidised by atmospheric oxygen, and suffers no change when the solution is warmed after the addition of a little sulphuric acid. On the other hand, in strong sulphuric acid solution benzaldehyde is unaffected by atmospheric oxygen and is readily sulphonated.

Since the reactivity of its different phases is different, it is necessary to bring a molecule into the correct phase before it can enter into a specific reaction. Almost without exception molecules when in the free state exist in non-reactive phases—that is to say, phases of too small energy content—and it is therefore necessary to supply energy to them in order to induce the necessary change in phase before the reaction will take place. It is a well-known fact that substances in the free state will not react together, even though the reaction when it does take place may be highly exothermic. Hydrogen and oxygen, hydrogen and chlorine, hydrogen chloride and ammonia, are instances which are familiar to everyone, and it is equally well known that if energy is supplied to these pairs of substances the reaction commences at once, this being due to the phase change which is produced by the energy supplied.

The difference in energy content of any two consecutive phases of a given molecule is one molecular quantum, and consequently the amount of energy required to convert a molecule from a non-reactive phase to a reactive phase must be 1, 2, 3, or 4 etc. molecular quanta depending on the relation between the two phases. This process will form the first stage of every reaction. When the phase change has taken place the molecule will react, one or more new molecules being produced. In this process, which is the second stage of the reaction, energy is lost, since otherwise the process would not take place. Each molecule formed in this second stage will be in the freshly synthesised state,

and therefore a third and final stage must ensue in which each newly formed molecule loses a definite number of its molecular quanta, and passes into that phase which is in equilibrium with the surroundings.

The second stage of the reaction, to which alone the ordinary equation applies, may be exemplified by the combination of hydrogen and chlorine. When these gases are mixed they have no influence on one another, since they both exist in non-reactive phases, but on the supply of the necessary number of its molecular quanta a molecule, say of chlorine, is converted into its reactive phase. Its molecular field is now in such a condition that it can form an additive complex, Cl_2,H_2. Since atomic energy quanta can be lost by the change $Cl_2,H_2 \rightarrow HCl+HCl$, this change takes place, and two freshly synthesised molecules of hydrogen chloride are formed.

The three stages of a reaction can be expressed in a general way by three equations :

$$1.\ A + E \text{ ergs} = A^1$$
$$2.\ A^1 = B^1 + F \text{ ergs}$$
$$3.\ B^1 = B + G \text{ ergs.}$$

In the first stage the molecule A absorbs the amount of energy E ergs, and is converted into the reactive phase A^1. In the second stage this activated molecule reacts to give the new and freshly synthesised molecule or molecules B^1 with evolution of the energy F ergs. In the final stage these newly synthesised molecules pass into their normal and non-reactive phase B with evolution of the energy G ergs. The observed molecular heat of reaction in the change from A to B is given by $F+G-E$ multiplied by the Avogadro constant, and is positive or negative—that is to say, the reaction is exothermic or endothermic, according to whether $F+G$ is greater or less than E.

It is evident that the energy E is equal to one or more molecular quanta characteristic of the molecule A, and that the energy G is equal to a whole number of molecular quanta characteristic of the molecule B. The energy F must also be equal to one molecular quantum characteristic of the molecule B, since, if the molecule A^1 loses any energy whatever, it will lose its reactivity and no reaction will occur. The recognition of these three stages of a chemical reaction and the quantitative expression of the energy change associated with each is of fundamental importance.

The three methods of supplying the necessary increment of energy E to produce the necessary phase change for activation of a molecule have already been discussed in detail. They consist in exposing the molecule either to infra-red rays of frequency equal to the atomic, intramolecular, or molecular frequencies of the molecule, or to light of frequency equal to that characteristic of the normal and non-reactive phase of the molecule, and in submitting the molecule to the influence of a solvent or catalyst. If the necessary increment of energy, E, is small, it may readily be supplied by the first or the third method. This is the more common case, and it explains why so many reactions are induced by rise in temperature and why so many take place in solution. When reactions take place in solution the first stage is apparently absent, but it must be remembered that in such cases the first stage occurs at the moment the solution is formed.

In order to obviate any criticism at this point, based on ionic reactions, it may be stated that ionisation is a property of a particular phase of a molecule, which is formed in solution. Nothing antagonistic to the phase theory is to be found in the fact that one of the properties of a particular phase is its resolution into ions. Mention may also be made of the undoubted reactivity of the non-ionised molecules which are present in the ionic equilibrium.

Again, some difficulty may be caused by a restricted use of the term 'solvent.' The molecules, which by virtue of their residual affinity cause the phase change on the part of the reactant molecule, are effective whether an actual solution is formed or not. The phenomenon is, in fact, one of catalysis in which the activating molecule acts as catalyst. A catalyst is, therefore, a substance the molecules of which by virtue of their residual affinity form complexes with the reactant molecules, and activate them by the supply of one or more molecular quanta to each. In heterogeneous catalysis the action of the catalyst must be

restricted to those reactant molecules which are in actual contact with its surface. The activated layer of reactant molecules can therefore be only one molecule deep, and this has been experimentally established.

In the case of highly endothermic reactions in which the amount of energy, E, required to activate a single molecule is large—that is to say, a large number of molecular quanta—considerable difficulty will be found in realising them when the energy is supplied by either the first or the second method. They can, however, be induced when the energy is supplied at the phase frequency, for one phase quantum is sufficient to change a molecule into any phase. Such reactions are grouped under the general term 'photochemical,' since the phase frequency always lies in the visible or ultra-violet region of the spectrum.

A very important deduction may be made from the existence of the first stage of a reaction—namely, that for every increment of energy, E, absorbed one molecule must undergo reaction. Since in all photochemical reactions the amount of energy absorbed per molecule is one phase quantum, the deduction in its simplest form states that in all photochemical reactions one molecule must react for every quantum of light energy absorbed. This is known as Einstein's law of photochemical equivalence, and was enunciated by him on the basis of Planck's energy quantum theory. This law can very easily be put to the test of experiment, but it has been found that in every case, except the photochemical ozonisation of oxygen, the number of reacting molecules is very much greater than one for every quantum of energy absorbed. In some reactions as many as 10,000,000 molecules react for every quantum absorbed. Not only has this extraordinary divergence given rise to serious criticism of the energy quantum theory, but it has even led to the view that there is no connection between radiant energy and chemical reaction.

The molecular phase theory gives a complete explanation of this divergence from Einstein's law—an explanation which has been experimentally proved, and which has considerably advanced our knowledge of photosynthetic processes in general. Indeed, the theory receives its greatest support from this phenomenon. The application of the theory to the problem is very simple, and merely takes into account the energy that is evolved in the second and third stages of the reaction. An exothermic photochemical reaction may be considered of a substance with phase quantum equal to 34 molecular quanta, and for which the necessary increment of energy, E, per molecule is 2 molecular quanta. On exposing this substance to light of frequency equal to its phase frequency, the molecules will each absorb one phase quantum of 34 molecular quanta and become activated. Let a single molecule absorb one such quantum and be converted into the active phase. For this phase change two molecular quanta are required, and therefore 32 molecular quanta are evolved. Since this energy is radiated at the characteristic infra-red frequencies of the molecule it can be re-absorbed by the surrounding molecules, so that under optimum conditions the absorption of one single phase quantum may cause the activation of 17 molecules. This is the first reason for the divergence from Einstein's law.

The instant that a molecule is activated it reacts, and the second and third stages of the reaction ensue with the evolution of the energy F+G. This energy is radiated as heat—that is to say, although it is equal in amount to a whole number of molecular quanta characteristic of the resultant molecules, it is radiated at the atomic and infra-molecular frequencies characteristic of these molecules. Whatever the actual reaction may be, the reactant and resultant molecules must possess some atoms in common, and therefore frequencies in common. The energy radiated during the second and third stages of the reaction of a molecule must be wholly or partly reabsorbed by the surrounding molecules, with the result that these become activated and react. This is the second reason for the divergence from Einstein's law. It may readily be understood that when E is small and F+G is large the divergence will become enormously great if the proportion of the radiated energy that is reabsorbed is large.

This proportion will depend on two factors, the density of the reacting substance and the density of the radiated energy; and if the phase theory explanation is correct, the divergence from Einstein's law will rapidly increase when either the density of the absorbing substance or the density of the radiated energy is increased. There is abundant evidence that the divergence from Einstein's law

does increase very rapidly with the density of the absorbing substance. With certain photochemical reactions in solution Henri and Wurmser (*J. de Physique*, **3**, 305 (1913)) found that at a concentration of $N/10$ 180 molecules react for every phase quantum absorbed, whilst at a concentration of $N/1$ this number is increased to 1,360. Still more striking is the fact recorded by Bodenstein and Dux, that the number of molecules of hydrogen and chlorine which react for every phase quantum absorbed by the chlorine varies as the square of the density of the chlorine.

The variation in the divergence from Einstein's law with the density of the radiated energy has also been proved in the case of the reaction between hydrogen and chlorine (Baly and Barker, *Trans. Chem. Soc.*, **119**, 653 (1921)). The density of the energy radiated during the reaction depends on the velocity of the reaction, and hence on the intensity of the incident light; the amount of hydrogen chloride, therefore, formed per minute was determined with different intensities of incident light. The phase theory demands that the amount of hydrogen chloride formed per minute should increase at a far greater rate than is accounted for by the increase in the intensity of the incident light—that is to say, the number of molecules of HCl formed for each quantum absorbed should rapidly increase as the intensity of the incident light is increased. Not only was this found to be the case, but two additional phenomena were observed which afford strong confirmatory evidence.

In the first place, with a constant intensity of the incident light, the velocity of the reaction is at first very small and then increases rapidly up to a constant maximum. It is evident that during the first instant the reaction will obey Einstein's law, but the reabsorption will then commence and the reaction velocity will increase until the proportion of the radiated energy that is reabsorbed reaches the maximum possible for the conditions existing.

In the second place, when the incident light is cut off the reaction stops instantly, but the chlorine reaches its normal condition very slowly. At least thirty minutes are required for the normal state to be reached, for if the light be again allowed to fall on the mixture of gases before this period of time has elapsed the initial velocity of the reaction is abnormally great, and the constant maximum rate is established sooner than is normally the case. The explanation of this very interesting phenomenon is to be found in the presence of partially activated chlorine molecules, *i.e.* molecular phases intermediate between the reactive and normal non-reactive phases. These intermediate phases, although they are not reactive towards hydrogen, contain more molecular quanta than the normal non-reactive phase, and therefore require less energy to convert them into the reactive phase. This partially activated condition of the chlorine constitutes one of the strongest pieces of evidence in favour of the molecular phase theory.

One further conclusion also follows from the above—namely, that in an endothermic reaction, in the second and third stages of which the energy radiated is very small compared with the critical increment, E, necessary for the first stage, Einstein's law should be obeyed. It is an interesting fact that in the photo-chemical conversion of oxygen into ozone, which is the only highly endothermic reaction yet studied quantitatively, Einstein's law is obeyed, since two molecules of ozone are produced for every phase quantum absorbed by the oxygen.

An exactly analogous explanation to that detailed above for the photochemical combination of hydrogen and chlorine is given by the phase theory for purely thermal reactions, such as the thermal decomposition of phosphine, in which great divergence from the Einstein law is observed. The number of molecular quanta absorbed per second by the phosphine by the radiation from the walls of the containing vessel is calculated from their temperature by the well-known laws of radiation. The number of phosphine molecules decomposed per second is measured and found to be some million times too large. Here, again, since the reaction is exothermic, the energy evolved in the second and third stages can be reabsorbed by the surrounding phosphine molecules. In fact, the decomposing phosphine molecules form a radiating system, with the result that the density of the effective radiation, *i.e.* the number of molecular quanta available per second, is very much greater than that calculated from the temperature of the walls of the containing vessel.

In this type of reaction there is another large source of error in the calculation. It is assumed that the energy is only absorbed by the reacting substance at its molecular frequency in the short wave infra-red, and it is the amount of energy radiated at this frequency only by the walls of the containing vessel that is used as the basis of calculation. This assumption of monochromatic absorption of energy by the phosphine is incorrect according to the phase theory, since energy can also be absorbed by the gas at its atomic and intramolecular frequencies, the smaller quanta thus absorbed being summed up within the molecule to form molecular quanta. The total number of molecular quanta gained by the gas per second is, therefore, much larger than that calculated from the radiation by the walls of energy of frequency equal to the molecular frequency of the phosphine. When the increase in reaction velocity due to this is taken into account, together with the resulting increase in the proportion reabsorbed of the energy radiated during the second and third stages of the reaction, the very great divergence of the observed results from those calculated can easily be understood.

An interesting result follows from the increase in the proportion reabsorbed of the energy radiated during a reaction caused by an increase in the reaction velocity. In an exothermic reaction, if this radiated energy is completely reabsorbed the surrounding molecules will be completely activated and will react. Under these conditions the absorption of a single energy increment, E, by one molecule will be sufficient to cause an infinite number of molecules to react, and the reaction will proceed as an explosion wave through the whole mass of the substance. The criterion of an explosive reaction is, therefore, that the proportion reabsorbed of the energy radiated during a reaction reach a critical limit, so that the surrounding molecules are activated. The existence of a limiting intensity of illumination below which explosive combination of hydrogen and chlorine does not take place is well known. Again, the effect of wire gauze in stopping an explosion wave in a gas is due to the absorption by the metal of energy quanta from the gas molecules, which thereby lose their critical increments of energy and therefore can no longer react.

It may be claimed that the phase theory receives strong support from the quantitative experiments described above. This support is further strengthened by an extension of the principle of the reabsorption of the energy radiated during a reaction. Let a substance A undergo a photochemical reaction when it absorbs light of frequency V_1; if a beam of light pass through a vessel containing A it will be deprived of all rays of frequency V_1 and will therefore produce no effect when it enters a second vessel containing A. If a substance P, which absorbs light of frequency V_2, and is not thereby photochemically changed, is mixed with A in the second vessel, this substance P will absorb rays of frequency V_2 and will radiate this energy at its molecular and atomic frequencies. If the two substances A and P possess the same molecular or atomic frequencies, this radiated energy can be absorbed by A, with the result that some molecules of A will be activated and will undergo the same reaction as they do when exposed to rays of their own phase frequency, V_1.

The first example of this type of reaction, to which the name of 'photocatalysis' has been given, was described by Daniels and Johnson (*J. Amer. Chem. Soc.*, **43**, 72 (1921)). These authors found that N_2O_5 is not affected by blue light, but that, if some NO_2 which absorbs this light is mixed with the N_2O_5, the latter is decomposed when the mixture is exposed to blue light. The energy absorbed by the NO_2 is radiated at its molecular and atomic frequencies, and, since the two molecules have the same atomic frequencies, some of this radiated energy is reabsorbed by the N_2O_5, which is thereby photochemically decomposed.

A second example of photocatalysis was found in the photochemical conversion of carbonic acid into formaldehyde. This reaction normally takes place when carbonic acid is exposed to light of very short wave-length, 200 $\mu\mu$, but it has been found possible by the use of a photocatalyst to realise this reaction with the aid of visible light only. The ideal photocatalyst will obviously be one which possesses exactly the same molecular frequency as the substance to be photocatalysed, and, as previously explained, this is secured by using a photocatalyst which forms an addition complex with the catalyst. In the case of carbonic acid an excellent photocatalyst is found in malachite green, which

forms in solution an addition complex with carbonic acid. On exposure of this solution to visible light formaldehyde is produced, the same result being obtained with other coloured basic substances as photocatalysts (Baly, Heilbron, and Barker, *Trans. Chem. Soc.*, **119**, 1025 (1921)).

These two examples are sufficient to demonstrate the reality of photocatalysis which was foretold from the phase theory. The formation of formaldehyde from carbonic acid possesses an added interest because this reaction undoubtedly constitutes the first step in the growth of living plants. As is well known, sunlight contains no ultra-violet light of such short wave-length as 200 $\mu\mu$, and consequently the CO_2 assimilated by the plant cannot be converted by sunlight into formaldehyde without some assistance. Willstätter has shown that the CO_2 absorbed by the living leaf combines in the form of H_2CO_3 with the chlorophyll. The chlorophyll therefore acts as the photocatalyst, and on absorbing visible light from the sun it radiates this energy again at its molecular frequency. Owing to the identity of the molecular frequencies of the two components of the complex this energy is reabsorbed by the carbonic-acid component, which thereby becomes activated and reacts to form formaldehyde and oxygen.

This photosynthetic production of formaldehyde has very important consequences, since the molecules when freshly synthesised possess a very remarkable reactivity. Formaldehyde exhibits an absorption band at the wave-length of 290 $\mu\mu$ or the frequency of 1.0345×10^{15}, and when it is exposed to light of this frequency each molecule absorbs one phase quantum, and is thereby converted into a phase of very high energy content. This phase is the same as that in which the newly photosynthesised molecule exists, for the two give identical reactions. One of these reactions is the combination with the potassium nitrite, which is always present in the leaf, to give formhydroxamic acid. This substance at once reacts with more activated formaldehyde to give α-amino acids, pyridine, piperidine, pyrrole, pyrrolidine, glyoxaline, quinoline, *iso*-quinoline, indole, and derivatives of xanthine. These reactions have been observed in the laboratory with both photosynthesised formaldehyde and photochemically activated formaldehyde. A second reaction of the activated formaldehyde is its polymerisation to form hexoses, and this reaction has also been realised in the laboratory with both photosynthesised and photochemically activated formaldehyde.

According to the phase theory these various substances, α-amino acids, nitrogen bases, and hexoses, are produced in highly reactive phases, and will, therefore, possess reactivities new and strange to those who only know the compounds in their normal and less active phases. The formation of substituted α-amino acids, like histidine, and the condensation of these to form proteins, is, therefore, not by any means extraordinary, nor is the condensation of hexose molecules to form cane-sugar and then highly complex starches and carbohydrates to be wondered at, for all these reactions are merely those characteristic of phases with greater energy content than the phases known to the organic chemist. Many of these reactions have actually been realised in the laboratory by the use of activated formaldehyde, and the following substances have been photosynthesised : Several substituted α-amino acids including histidine, three alkaloids including coniine, and a mixture of carbohydrates as yet not identified, but probably of greater complexity than cane-sugar (Baly, Heilbron, and Hudson, *Trans. Chem. Soc.*, **121**, 1078 (1922)).

The phenomenon of photocatalysis and its applications afford the strongest evidence yet found for the theory of molecular phases. Although it may be argued that it has little to do with absorption spectra, such argument can only be inspired by misunderstanding, for it is entirely based on the frequencies which characterise substances. Every molecular phase is characterised by its own energy content, its own absorption frequency, and its own chemical reactivity. Evidence for the reality of these phases is derived from energy relationships, from absorption spectra, and from chemical reactions. The fact that the chemical arguments, though based on the absorption of energy at the characteristic frequencies of molecules, are not in every case completed by a knowledge of the absorbing frequencies of all the phases that take part in a reaction, does not detract from those arguments. When a molecule is converted by the supply of energy into a phase which reacts, that phase has only a transient existence, and there is as yet no means of identifying it by its

absorption spectrum. The chemical arguments cannot as yet, therefore, be entirely completed by absorption measurements, but it is only the identification of the reactive phase which is now lacking, and that by no means in all cases. The initial phase of the reactant molecules, the final phase of the resultant molecules, and the molecular quanta of both can readily be measured by absorption spectra methods.

This lacuna, however, only concerns one argument, and is one of secondary importance. The evidence now at hand in favour of the phase theory would seem to be strikingly convincing. Let it always be remembered that this theory is but an extension of Planck's great theory, the enunciation of which marked the beginning of a new epoch in chemistry.

Stratigraphical Sequence and Palæontology of the Old Red Sandstone of the Bristol District.—*Report of Committee* (Dr. H. Bolton, *Chairman*; Mr. F. S. Wallis, *Secretary*; Miss Edith Bolton, Mr. D. E. I. Innes, Professor C. Lloyd Morgan, Professor S. H. Reynolds).

A preliminary examination of the chief exposures of the Old Red Sandstone in the Bristol District has been made, and particular attention paid to the shore exposures at Portishead, where Professor S. H. Reynolds has identified the locality from whence Dr. Martyn obtained fossil fish remains.

The fish remains of the Old Red Sandstone in the Bristol Museum have been examined and, from the character of the matrix, it is evident that these fossils occur on at least three horizons. An investigation of the mineral constituents of the Old Red Sandstone is now in hand, and a large number of rock specimens have been collected for this purpose.

Owing to the scarcity of the fish remains in the Portishead deposits it is proposed to obtain permission to quarry, and thus open up new exposures of the zone. For this purpose, and also for the preparation of specimens for further mineralogical examination, a grant of 15*l.* is hereby applied for.

Naples Table.— *Report of Committee* (Professor E. S. Goodrich, *Chairman*; Professor J. H. Ashworth, *Secretary*; Dr. G. P. Bidder, Professor F. O. Bower, Dr. W. B. Hardy, Sir F. S. Harmer, Professor S. J. Hickson, Sir E. Ray Lankester, Professor W. C. McIntosh, *the late* Dr. A. D. Waller) *appointed to aid competent investigators selected by the Committee to carry on definite pieces of work at the Zoological Station at Naples. Drawn up by the Chairman and Secretary.*

The recent meetings of this Committee indicated that the continuance of the Table was considered to be desirable, and on behalf of the Committee we apply that the Committee should be reappointed with a grant of £100.

The grant of £100 made for 1922 has been paid to the Director of the Stazione Zoologica, and nothing remains in hand.

From March 27 to April 22 Mr. G. R. de Beer occupied the British Association Table, and submits the following report of his work:—Examined Cephalopods for the earliest stages of Dicyemids; obtained stages of a Sporozoan parasite of Sipunculus; experimented with poisons on larvæ of Strongylocentrotus and Astropecten; regeneration experiments on Sponges; studied the formation of a semi-permeable membrane in collar cells; made preparations for the study of the gametogenesis of Phyllirhoe; and obtained material for the study of some points in the development of Petromyzon.

The Table was occupied during part of June and July by Professor E. S. Goodrich and Dr. Helen Goodrich, who respectively report as follows:—

Professor E. S. Goodrich: I occupied the British Association Table at the Stazione Zoologica in Naples during part of June and July 1922. The staff

showed me every kindness and displayed great zeal in obtaining the material I needed. My chief object was to study the development of the nephridia in the later larval stages of amphiosus—stages which have hitherto been very difficult to obtain in Naples. After initial failures we eventually found larvæ in abundance, and I hope soon to be able to publish the results of my researches on living and preserved specimens. I also obtained other material, including the interesting worm *Bonellia viridis*.

Dr. Helen Pixell Goodrich: During part of June and July this year I occupied the British Association Table at the Stazione Zoologica, Naples. Plentiful material was obtained for me in excellent condition for work on parasitic protozoa. Two species of Holothuria were found to have several more parasites than have been previously described, and several specimens of two rather rare species of Synapta were also obtained for me to examine. In addition to this, I continued some investigations on Gonospora glyceræ from Rhynchobolus, these having been in abeyance since early in the War owing to the impossibility of obtaining material.

Some other Polychæta, also Bonellia, were examined.

The results of these investigations will, I hope, be ready for publication in the near future.

Zoological Bibliography and Publication.—*Report of Committee* (Professor E. B. Poulton, *Chairman*; Dr. F. A. Bather, *Secretary*; Mr. E. Heron-Allen, Dr. W. Evans Hoyle, Dr. P. Chalmers Mitchell).

Correspondence in *Nature* on the excessive cost of scientific periodical publications induced the Secretary to write a letter pointing out that adherence to the recommendations of this Committee would frequently reduce the cost of printing. This led to application for the Reports and Circulars of the Committee from many to whose notice they were thus brought for the first time. Several copies were dispatched, especially to India.

The attention of a few editors has been drawn to the principles and methods advocated by the Committee as the need has seemed to arise. Some editors are curiously reluctant to edit their authors. One editor, for instance, protested that he could not ask his contributors to add to the titles of their papers a word explaining the class of the animal kingdom to which the subjects of their description belonged. Many of his contributors, we observe, do this, presumably of their own accord, and we doubt not that the others would follow their example were its usefulness pointed out to them. Indeed, our experience is that most authors are grateful for advice of this kind, and err only from ignorance of the better way. But as the Committee cannot approach every individual author, it must rely on the help of editors.

In all the matters for which the Committee has from time to time desired the attention of authors and editors there is a gradual assimilation to the practice that it recommends. Most new publications follow the recommendations, and this confirms us in our belief that we are working on the right lines. There is, however, still a field for our efforts. Authors' reprints, for example, are still repaged in many cases, and are too often distributed without the correct date or other bibliographic details. In a paper thought of such importance by its author as to be issued three times in various forms there was no indication that several of the generic and specific names were being used for the first time. It was not till several days' search failed to find one of them in previous literature that this fact was suspected and then confirmed by inquiry of the distant author. How many weary hours might be saved to scientific workers if authors would but give five minutes to notifying these small details!

Your Committee, therefore, asks for reappointment, with a grant of 1*l*. to cover postage and minor printing expenses. It returns an unexpended balance of 17*s*. from a similar grant last year.

Parthenogenesis.—*Report of Committee* (Professor A. MEEK, *Chairman*; Mr. A. D. PEACOCK, *Secretary*; Mr. R. S. BAGNALL, Dr. J. W. HESLOP HARRISON).

THE objects of this research were investigations into the biological and cytological phenomena of parthenogenesis in sawflies of the family of *Tenthredinidæ.*

Some forty species have been collected wild or obtained through the kindness of Miss E. F. Chawner, Lyndhurst, Hants, and reared in the laboratory in order (1) to develop a technique, (2) to acquire diverse material for ascertaining how far parthenogenesis is a feature of sawfly biology, and (3) to make a selection of species particularly amenable to experiment.

Attention is being particularly focused on the following species : *Allantus* (*Emphytus*) *pallipes* Spin.; *Pristiphora pallipes* Lep., which produce only females by parthenogenesis, and *Crœsus septentrionalis* L.; *Pteronidea ribesii* Scob., which produce only males parthenogenetically, but the species *Nematinus luteus* Panz., *Holocneme lucida*, *Ametastegia* (*Taxonus*) *glabrata* Fall., *Phymatocera aterrima* Kl., *Pteronidea pavida* Lep., *P. melanaspis* Htg., *Thrinax mixta* Kl. and *T. macula* Kl. are also under close observation.

The results to date may be summarised as follows, the observations on parthenogenesis being noted first as they form the object of the inquiry :—

Parthenogenesis.

(*a*) *New Parthenogenetic Species.*—The following seven species as additions to the lists of Cameron and Enslin are brought forward as facultatively parthenogenetic : (1) *Nematinus luteus* Panz., (2) *Nematinus abdominalis* Panz. nec F., *Platycampus luridiventris* Fall., (4) *Thrinax mixta*, (6) *Pteronidea poecilonota* Zadd., (7) *Dolerus aeneus.* Htg.

As experimentation proceeds others will undoubtedly be added.

(*b*) *Number of Consecutive Parthenogenetic Generations.*—*Allantus* (*Emphytus*) *pallipes* has been reared for four generations and *Pristiphora pallipes* for four generations. In both cases no male was reared. The strains are being maintained.

(*c*) *Pairing and Egg-laying Habits.*—The most important breeding results to date concern observations on *Phymatocera aterrima* and *Nematinus luteus.* The females of both species, after laying eggs parthenogenetically, were found to pair with the male. After such laying and pairing more eggs were obtained from *N. luteus*, but, owing to the enforced use of alder twigs instead of young trees (the supply having been exhausted), this second batch of eggs, presumably fertilised, was not hatched because the twigs wilted. The females of *P. aterrima* were observed to be almost spent before pairing, and their failure to lay a second, and possibly fertilised, batch of eggs did not occasion surprise.

These experiments suggest that the same female sawfly may reproduce both parthenogenetically and sexually, dependent upon its rejection or acceptance of the male. Further work is proposed this summer and autumn, utilising suitable species, with a view to ascertaining (1) how far this practice obtains among sawflies; (2) how it affects the production of the sexes and *ipso facto* the sex ratio; (3) the chromosome complement of offspring resulting from the two different batches. Should the practice be widely spread in this family, much light will be shed upon other curious phenomena among sawflies, viz. the ignoring of the males by the females (or *vice versa*) in certain species; the parthenogenetic production of males only in certain species; the rarity of males in certain species.

Gametogenesis.

A large amount of material—about eighty tubes—has been preserved for cytological work on the species under particular observation. (See above.)

The labour of carrying on breeding experiments during the season, May-September, forbids much else than the collection of material, but a certain number of slides illustrating oogenesis in *Allantes pallipes* and *Pristiphora pallipes* and spermatogenesis in *Thrinax mixta* have been made and preliminary readings noted. Certain of the gross features of oogenesis and the behaviour of the nucleolus in regard to budding have been observed, but the finer work on chromosomes has barely been touched.

Anatomy and Biology.

Among others, new facts have been elucidated, *en passant*, as follows :—

1. Life histories and habits of *Allantus pallipes*, *Pristiphora pallipes*, *Thrinax mixta*, and *T. macula*. The two first species have been studied in collaboration with Miss Chawner.
2. Development of the egg of *Allantus pallipes* and *Pristiphora pallipes*.
3. Presence of an inflation apparatus in various species.
4. Condition of the fat body and silk glands during different stages of life history.

Papers.

A paper on 'Observations on the Biology of Sawflies' by the Secretary of this Committee, and another entitled 'Observations on the Life History and Habits of *Allantus* (*E.*) *pallipes* and *Pristiphora pallipes*,' in collaboration with Miss E. F. Chawner, have been forwarded to the *Entomologist* for publication. Later a communication, 'Pairing and Parthenogenesis in Sawflies,' was addressed to the Editor of *Nature*.

Experiments in Inheritance of Colour in Lepidoptera.—

Report of Committee (Prof. W. BATESON, *Chairman*; The late Hon. H. ONSLOW, *Secretary*; Dr. F. A. DIXEY, Professor E. B. POULTON).

Diaphora (*Spilosoma*) *mendica* and var. *rustica*.—As was reported last year, these experiments were concluded, and the full report has since been given (*J. Genet.*, Vol. XI., No. 3, December 1921). The great variation in the buff-coloured hybrids is probably due to the effects of modifying genes. The colour of these buff insects, as well as that of the others, was measured by the tintometer, and distribution curves made, so that the segregation of these variable insects might be followed without difficulty. A consideration of what is known concerning the chemistry of black pigments supports the statement that var. *rustica* is phylogenetically the older race.

Boarmia consortaria and var. *consobrinaria*.—No further experiments were considered necessary.

Hemerophila abruptaria and var. *fuscata*.—These experiments being concluded, the full report was written and published (*J. Genet.*, Vol. XI., No. 3, December 1921).

Zygæna filipendulæ and the yellow variety.—The few insects emerging last year confirmed the conclusion that the red colour is dominant to yellow, but as the numbers were considered too small, further pairings were made last July, but none of the insects have as yet emerged.

Abraxas grossulariata and var. *varleyata*.—As was stated in last year's report, these experiments have been completed, and a full account has since been published (*J. Genet.*, Vol. XI., No. 2, September 1921). A method was described by which the black pattern could be measured, its area being then expressed as a percentage of the total extent of the fore wings. The values of the black pattern in several families were obtained, and were plotted as distributions and as percentage-frequency distributions.

Abraxas grossulariata and var. *exquisita*.—As it was expected that *exquisita* was produced by the combination of *varleyata* and *lacticolor* in the same insect, matings were made to test this hypothesis, and, in addition to figures already obtained in the autumn, a very large number of insects have emerged this summer. The results have not yet been tabulated, but a full report will, it is hoped, appear in the *Journal of Genetics*, and may throw some light on the question of anomalous sex ratios observed in previous crosses *grossulariata* × *varleyata*, *grossulariata* × *lacticolor*, and *grossulariata* × *exquisita*.

The Effects of the War on Credit, Currency, Finance, and Foreign Exchanges.—*Report of Committee* (Professor W. R. Scott, *Chairman*; Mr. J. E. Allen, *Hon. Secretary*; Professor C. F. Bastable, Sir E. Brabrook, Dr. J. H. Clapham, Dr. Hugh Dalton, Mr. B. Ellinger, Sir D. Drummond Fraser, Mr. A. H. Gibson, Mr. C. W. Guillebaud, Mr. F. W. Hirst, Professor A. W. Kirkaldy, Mr. F. Lavington, Mr. D. H. Robertson, Mr. E. Sykes, Sir J. C. Stamp).

Last year the Committee discussed thirteen questions which proved controversial. We were unable to make unanimous recommendations, and presented a Report which was largely a symposium of individual opinions. This year we have discussed only seven questions, and have aimed at the greatest common measure of agreement.

Question 1.—What are the principal differences in the economic influence of Inter-Ally Debts or German Reparations and Indemnities in the late War as compared with previous Wars? What would be the result of cancelling or reducing—(a) Inter-Ally Debts? (b) German Reparations and Indemnities?

The most obvious difference is that of the amount of the obligations compared with the productive power and resources of the debtor countries. In some cases something might turn on the datum taken, as, for instance, should it be the pre-War capacity (in the case of a country which has sustained much war damage) or the present capacity (which is in most cases uncertain) or the calculated future capacity (which is much more uncertain)? Further, as their size is also extremely great in relation to the productivity of the recipient countries, the disturbance to those countries' industries at the initiation and termination of the period of receipt of the annual payments of goods, etc., is correspondingly great. Moreover, so far as Germany and Great Britain are concerned, the industries of these countries are *competitive* and not *complementary* to an unusual extent. A further difference may be found in the extension of credit; the greater commercial interdependence of nations in the pre-War system tends to accentuate the initial effects of the disturbance caused by payments of indemnities, of interest and of principal; at the same time it is probable, or at least possible, that the credit system is capable of developing compensatory action by which such consequences would be counteracted. If we turn back to the nearest parallels which history offers and compare the late War with the cognate balance-of-power wars against Louis XIV. and Napoleon, we find that Louis XIV. was never left at the mercy of the Allies as Germany has been, and that in the case of Napoleon the consummation of the war was a restoration of the French monarchy, which had then to be treated as a friendly Power.

Although more than three and a-half years have passed since the Armistice, the amounts to be paid by Germany in respect of reparations and indemnities are still indefinite. Questions which ought to have been considered and decided in accordance with common-sense and economic law have been confused by electioneering and political exigencies. No unbiassed observer imagines that the enormous sums imposed by the Peace Treaty can ever be paid by Germany, especially a Germany which has lost overseas possessions as well as territory containing great mineral wealth both on the eastern and western frontiers. Up to the present it is uncertain how far the actual money payments made by the German Government exceed (if they do exceed) the cost of the Armies of Occupation.

Note.—Sir Drummond Fraser dissents, holding that Germany is able to pay her full reparations 'if a genuine business proposition were placed before her drafted by business men and not politicians, *plus* academicians. Had the case been reversed Germany would not only have put a sound business proposal before the Allies, but she would also have exacted the uttermost farthing for reparations.'

(*a*) Four alternative plans offer themselves in place of exacting full payment, which appears never to have been contemplated on either side :—

1. Granting delay in payment of interest and instalments.
2. Reducing an agreed rate of interest.
3. Reducing the principal.
4. Forgiving interest for a specified number of years.

Obviously these alternatives only apply to war debts contracted by the respective Governments. Also a debt is either good, bad, or doubtful. It is fallacious to treat these three as if they were the same, and at present there is no criterion to decide as to which debts fall in either of the last two classes. In so far as interest is merely postponed, it appears to have the effect of temporarily alleviating the financial position, but the ultimate effect is likely to be adverse. A reduction in the agreed rate of interest or a proportionate reduction in the principal would have varied effects according to the conditions under which it took place. If either arose from the opinion of the creditor Government that it could not get more, this would be adverse to the credit of the debtor country, which would be, in effect, in the position of having to make a composition with its creditors. Therefore its rate for borrowing would move against it as contrasted with countries which did not require to compound. On the other hand, if a general reduction, or even a cancellation, were made by the United States, with Great Britain participating as an act of equity and friendliness, no adverse effects need be anticipated.

Needless to say the circumstances considered are apart from the offsetting of debts as between the late Allies. This is obviously desirable, but it is not easy at present, owing to the doubtful nature of some of these debts. The important country for remission of debt is the United States; for offsetting debts it is Great Britain. Probably from the British point of view the time would not be ripe till a reasonable valuation can be put on the debts owed to us. It is suggestive that the worse the financial administration has been in the past, the more ready we were to accept foreign obligations to this country as irrecoverable.

Note.—Sir Drummond Fraser objects to any reference to our debt to the U.S.A. except on this basis of payment.

Cancellation of 'good' debts, such as ours to the U.S.A., would evidently be likely to benefit the debtor country by raising its exchange, improving its credit, and relieving its internal financial difficulties—thereby hastening its industrial recovery and that of other countries indirectly. It would be likely to spare the creditor country the temporary disturbance to its industries arising from the annual receipt of large quantities of goods forming the interest and redemption payments. When these payments are actually made (there has been little attempt to make them hitherto) they cause unpaid-for exports in the debtor country and gratuitous imports in the creditor country. Should the payments be stopped suddenly by cancelling the balance of the debts, the men employed in the export trades in the debtor countries would find their occupation gone, and the same kind of thing would happen to the men in the creditor countries who had been employed by the specified purchasing power hitherto free because of the receipt of the gratuitous imports; there are certain to be effects of dislocation through the sudden disturbances of supply and demand in the home market. So much unemployment might follow on both sides that neither creditors nor debtors would be willing to wipe the slate. Where no payments have been made the considerations point to the desirability of cancellation without delay. At present the debts cause little inconvenience because no attempt is being made to pay even the interest, except in the case of the payment of interest to America promised by the British Government in the second half of the current financial year. It seems reasonable that the Inter-Ally debts—contracted in food and munitions at exorbitant prices—should be scaled down, and the creditor country should agree to receive interest in tariff-free goods of the debtor country.

(*b*) The large payments demanded by the Allies from Germany on account of reparations and indemnities have aroused much controversy in this country, and it is difficult for this Committee to take sides. It is to be regretted that the amount of these payments, which involve difficult questions of legal interpreta-

tion and assessment, could not have been fixed with something like the assent of the debtor country. If German indemnities were reduced to a level at which they were generally recognised as within her capacity to pay, her bonds might be saleable abroad and capital might be available for, *e.g.*, France and Germany to balance their Budgets and remove many of the hindrances to industrial recovery.

NOTE.—Sir Drummond Fraser adds : 'The Governments which are entitled to hold German Reparation bonds should, if required, issue their own bonds secured by the German bonds, and should use the proceeds for the reconstruction of the devastated areas, and/or in repayment of advances already made. I am sure that such bonds issued, for example, by the French Government would be acceptable to Americans and to nationals of other lending countries. This would enable France to balance her Budget by foreign instead of internal borrowing. The foreign borrowing would be for productive purposes. The cost of this borrowing should be spread over a number of years. The annual interest and repayment would be more than covered by the interest and sinking fund from Germany, secured by pledged assets and the requisite control. The extent to which exporting countries could participate would depend upon their ability to supply goods and services and not solely upon their ability to absorb foreign securities out of surplus savings.'

Much complaint has been heard of price-cutting and competition through German imports to this country in payment of reparation claims. Mr. G. Bernard Shaw writes : 'The plunder of the Germans is a mistake from every point of view. The only possible effect on the victors is to pauperise them. The Germans cannot pay in gold; and in whatever other commodity they pay the trade in that commodity will be destroyed in the country of the plunderers. Already our shipbuilding and mining industries have been devastated in this way; and when all the discharged shipwrights and miners have found parasitic employment in hotels, garages, kitchens, and barracks, the country will have become economically dependent on Germany, who will soon be strong enough to withdraw her support and leave us without either industries or characters.'

It is not certain, however, that the competitive power of Germany as an exporting country would be lessened if her liabilities on account of reparations and indemnities were cancelled or reduced. A tradesman or merchant sometimes sells his stock cheap because he is pressed by creditors, and must give them cash; but in the long run the trader with ample capital and good credit can offer the best value to his customers. The stimulus to exports given by a depreciating mark is not a healthy or lasting one, because it is gained at the expense of some portion of the community which cannot be permanently maintained, *e.g.* at the present time, the expense of the wage-earner who is reduced below an economic level of reward.

Question 2.—What steps should be taken, during the next few years, to deal with the National Debt? Is it imperative that redemption of the Debt should proceed continuously, and upon a large scale? Or should it be effected out of possible surpluses at the end of the financial year?

The apparent ease in the financial situation is largely illusory. It has been temporarily relieved by the resources liberated from industry by the exceptionally severe depression Therefore this temporary ease is quite abnormal. The main points of difficulty are (1) the floating debt, (2) the foreign debt, (3) the method of funding. The evils of the first are well known, and the disadvantages of the second are obvious; therefore it is sound policy to endeavour to reduce both as fast as possible. The cause of temporary ease of money makes it advisable for those controlling such capital to keep it liquid; therefore much of the money in Treasury bills is not available for funding operations. Accordingly, the funds required to reduce the floating and foreign debts must come either from new savings or from the Budget. In the relatively short period under consideration both methods must be used, and the policy of debt reduction through the Budget must always be kept in view. In the present time of depression, and in view of the moderate progress already made, it seems necessary to restrict this year's contribution to the Budget surplus, almost certainly a dubious result which may mean negative debt redemption. At the same time, had there been adequate economy in Government expenditure there would have been a surplus for a respectable reduction of debt this year.

Therefore we regard reduction of debt as most intimately related with economy in public expenditure. With economy reduction on a satisfactory scale is possible; without it such reduction will always remain subject to doubt.

There is some room for difference of opinion in the question of *immediate* debt redemption. It may be argued that the burden of taxation is so heavy at a time of depression like the present that it is better to trust to possible surpluses until trade revives once more. Certain loans have special sinking funds attached to them, which no doubt helped the original flotation, and still keep up the market price; it is doubtful whether this money required for these redemption payments should be met by borrowing even in a bad year. If a Government is bound to provide so many millions a year for specific sinking funds it is forced to retrench somewhere. For the National Debt as a whole the only real sinking fund is a surplus of revenue over expenditure; the specific funds attached to special loans are illusory if the Government can meet them by further borrowing. Until recently no one could have doubted that it always pays to reduce debt in peace time. A common argument for early debt reduction is based on the contention that as long as prices are above the normal level there is an advantage in debt redemption at those higher prices, in so far as fewer goods have to be surrendered to effect a payment of a specified amount of debt expressed in money. There is a certain amount of truth in this, in so far as the enhanced prices are wholly due to inflation of the currency. But it seems to us it is not the whole truth. The burden of the payment of debt in goods is also related to the total quantity of goods comprising the national dividend. As that quantity increases the burden of debt redemption (as also of the interest on debt) decreases, *i.e.* if it be assumed that interest and debt redemption require in goods one-tenth of the national dividend, and if at a later date that dividend in goods is increased by one-third, then the proportion of such dividend required to meet the same debt charge will be reduced from one-tenth to about one-sixteenth.

In the near future the real argument for steady reduction of debt is the improvement of the national credit, which will enable conversion of debt to be effected on more favourable terms. In our opinion the method of the 3½ per cent. Conversion Loan was fundamentally bad. Though it transferred short-dated into a long-dated obligation, the great increase in amount of debt to be redeemed, ultimately, was almost indefensible. The present policy should be not to rush conversion, but to have patience and to work towards such a position as will make a future conversion possible on a less interest charge without addition to the nominal amount of debt.

NOTE.—Mr. Gibson adds: 'In all long-period loans the Government should reserve an early date of optional redemption. In my opinion, in the absence of a further great war British Government credit will be on a 4 per cent. basis within three and on a 3½ per cent. basis within ten years.'

During the past twelve months 455 millions of short-dated Government debt (bills) has been replaced by a longer-dated Government debt (bonds) without adding to the dead-weight debt. The present Government policy of replacing maturing debt and statutory obligations for the redemption of debt by bonds should be continued until the time for the big funding loan arrives.

If we compare the figures of the National Debt on March 31, 1919, with the figures of August 19, 1922, it becomes clear that the portion which requires immediate consideration is the bond and bill debt (with the Savings Certificates).

	Aug. 19, 1922 Mil. £	March 1919 Mil. £
Funded Debt and Annuities	332	340
War Loans	2,048	2,145
Victory Bonds and Funding Loan	751	—
Conversion Loans	479	—
	3,610	2,485
Bonds and Bills	3,025	3,705
Foreign Debt	1,081	1,292
	7,716	7,482

Our Questions 3, 4, 5 and 7 are connected with each other.

Question 3.—Should the volume of the currency be deliberately restricted with the object of returning to an effective gold standard?

Taking a long view, the Governmental manipulation of currency has been, and, as far as can be foreseen, is likely to be mischievous. But we have to face the results of such manipulation, and therefore, recognising this, the aim should be to work towards an ultimate liberation of the currency from Governmental interference with the minimum of disturbance. Accordingly, further contraction of currency should be gradual until an effective gold standard is restored. This may come without much further deliberate control of the currency, but so many factors are involved that no precise conclusion can be reached. The gold position, and that of crops and of trade, are all-important elements. The precise point at which a revival of trade will manifest itself in relation to the volume of currency and to the amount of gold production at that time will be important. If things are left to themselves trade is likely to be rather feverish, with alternations of short-lived booms followed by longer depressions. It would seem to follow that a policy which would check the boom period would be advantageous in the long run. The great need of financial reconstruction is stable conditions, and so more is to be gained by the avoidance of extreme oscillations than would be lost by checking a temporary extreme business activity. To attain this end it is really more important to act on credit than on currency, and accordingly the banks can do more than is possible by the Government.

Note.—Mr. Hirst adds: ‘As experience shows that no Government can be trusted with the control and manipulation of a currency, the only sound currency is an automatic gold, or silver, or gold-silver (symmetallic) currency, with complete convertibility of notes and free trade in bullion.’

In the meantime the manipulation of currency by the Government is likely to be clumsy. It tends to be like moving the regulator of a watch with a pitchfork. Therefore the most efficient action is likely to be that of the banks. The recent amalgamations have made concerted action easier in some respects, perhaps not in others. If the banks, as a whole, could be brought to see the advantage of joint action in the control of credit at the beginning of a period of active trade considerable permanent advantages would be likely to result.

Perhaps the Currency Note Issue should be taken over by the Bank of England (as we suggested in our 1915 Report), with a consequent fusion of the two gold reserves.

Note.—Sir Drummond Fraser suggests that emergency issue of currency might be allowed if covered by self-liquidating securities against a ‘fine,’ plus an increased Bank rate, the extent of the emergency issue to be regulated by the proportional increase in the ‘fine’ and the Bank rate. He reckons that the legal-tender money circulating outside the British banking system—not required for retail payments—is £200 million. Special efforts should be made to attract this money as well as new money (savings) into Government securities, on the same principle as that of deposit banking before the War. He urges that cheques should be free from stamp duty (as in America) in order to economise legal-tender money and to make the central gold reserve more effective.

So far as currency is regarded as reacting on prices, it may be sufficient if we are content to restrain the rise in prices in this country during the latter phases of the next trade boom. It may be difficult to restrain the rise of prices earlier without cutting off England from her share in the recovery of world trade. Some of us are inclined to prefer the widening of the gap between sterling and gold to an unnecessary prolongation of the industrial stagnation.

It may happen that the level of gold prices in the world will permanently rise before long, and that this rise in gold prices, being greater than the rise in sterling prices, will suffice to bring the £1 to its old gold parity. We must, perhaps, wait and see what happens, and adjust our policy accordingly. A very great deal seems to depend on the policy of the U.S.A. Federal Reserve Board: on whether they permit a large or only a small rise in gold prices.

Question 4.—To what extent should a deliberate policy of restriction be left to the automatic action of the Banks; to what extent to undefined central

control by the Government of the day; and to what extent to definite statutory powers of the Government?

The policy of restraining the rise in home prices during the next boom requires, in our opinion, concerted action by the joint-stock banks. It must be admitted, however, that these banks failed to use their influence after the Armistice, when restriction was desirable; bank credit expansion proceeded almost unchecked until the early months of 1921. Our former gold standard acted as a powerful brake on Governments and banks when they were tempted to cross the bounds of sound finance.

It is desirable that there should be a further limitation in the fiduciary issue of currency notes, and it might be well if the issue were steadily reduced; such a reduction would tend to check incipient inflation by compelling an increase in the Bank rate.

We do not think that the Government of the day should have any increase in its present powers. The Treasury has at present ample powers over the Currency Note Issue, and we see no need of the grant of further statutory powers.

Question 5.—When do you expect the dollar exchange to reach par? When it does: (a) Shall all restrictions on the movement of gold coin and bullion and on the melting of coin be removed at once? (b) What restrictions, if any, other than that imposed by convertibility into gold coin, should be imposed on the issue of currency notes?

If the Federal Reserve Board permit a rapid and large rise of gold prices in the U.S.A., the exchange might come to par midway in the course of the next trade boom. But the prospect is highly uncertain, and depends upon many arbitrary factors which are at present quite indeterminate.

In addition to the uncertainties which affect the position in this country there is the fact that the London money market is used as the medium for exchange transactions between Europe and the United States. Disturbances in the European exchanges tend to be reflected in the quotation of the £ in New York. Also there is the effect of American monetary policy in the interim. Therefore the date of the dollar exchange reaching par is something to gamble on, not on which to express a reasoned opinion. As regards European currencies, ultimate devaluation will, in most cases, be inevitable. It is too soon yet to take steps, since the problems of balancing Budgets, of the respective national credits, of war debts, and of reparations must first be dealt with. The question is, At what stage can these countries get back to a gold basis? The latter is urgent, but it seems that it cannot be effected without devaluation.

The U.S.A. might help by the redistribution of some of her gold by means of gold loans to countries which are prepared to balance their Budgets and seriously tackle their currency problems.

NOTE.—Mr. Hirst thinks that 'the sterling exchange could be restored to par by contracting the issues of our paper currency.'

5 (*a*) and 5 (*b*).—Questions 5 (*a*) and 5 (*b*) aroused differences of opinion. The most prudent answer to 5 (*a*) seems to be—' Only if conditions seem to be so favourable that we can re-establish a gold standard without grave risk of having our gold reserves too heavily depleted. Such a policy might be very risky if gold prices were likely to fall heavily or if our balance of trade seemed likely to become unfavourable.

In answer to Question 5 (*b*) we are inclined to repeat our suggestion that the issue should be transferred to the Bank of England, and regulated by fixing a maximum legal fiduciary issue in a manner similar to that of Bank of England notes. Whether the notes should be convertible into sovereigns or not is a doubtful point. Probably they should be convertible only into bullion in order to avoid the heavy cost of again using gold as a currency.

Question 6.—Is disarmament, i.e. a drastic proportionate reduction of European armies and navies, necessary in order (1) *to remove deficits, and so* (2) *to stabilise currencies?*

On the whole excessive and competitive armaments cause more political insecurity than they cure, while they also make it impossible to balance Budgets and stabilise the currencies of countries without public credit. A general arrangement for reducing military Budgets to a police level is perhaps the

chief need of Europe. We are not sure, however, whether the Army should be proportioned to the population, or the cost of the Army proportioned to the tax revenue.

Whether a country can maintain its present or any given standard of armaments, balance its Budget, and stabilise its currency depends upon its output of commodities and the distribution of the output between the Government (including the Army and Navy) and the civilian population.

The unsettlement of the War and the consequences of making a fetish of nationality at the subsequent Peace Conference have resulted in a good deal of political instability, not only externally but also internally. Accordingly, in many countries, for the present the necessary military forces are in excess of those that should be required in a few years. On a broad view of the situation serious internal disturbances in any country would be inimical to the general interest of Europe. Therefore in such cases the maintaining of an adequate force takes precedence of Budget adjustment or stabilising the currency. In several cases forces largely in excess of this necessary minimum are maintained. But that reduction should not be *proportionate*, if proportionate means that disarmament is to be calculated as the reduction of present forces by an equal proportion. That would simply be to put a premium on the action of those countries which had been most extravagant in maintaining large forces. They would only lose men that it would soon turn out they could not maintain as effectives; whereas another country, which had already brought its forces to a minimum, might possibly be driven below the margin of safety. It seems, probably, that several countries will have to apply the pruning knife themselves, being driven to it by financial exigencies. This is possibly an argument against an immediate cancellation of war debts and war obligations.

Question 7.—What is your opinion on the suggested devaluation of certain European currencies?

On this question our opinions differ. Evidently there are some currencies—the rouble, the Austrian crown, the Polish mark and the German mark—which have lost all relation to their pre-War value, and must undergo a process of devaluation. There are other currencies, such as the Greek drachma and the Italian lira, which still have a substantial value, though it does not seem conceivable they should ever again be worth 25.22½ to the golden sovereign.

That the rouble, the crown, and the Polish and German marks must be scaled down to an enormous extent if they are to have any relation to gold seems beyond question. The complete recovery of the drachma and the lira appears highly improbable; but the real problem is that offered by the French (and Belgian) franc.

On the whole we think that the policy of the Bank of France, aiming at the ultimate restoration of the franc to its old gold parity, is an unwise and probably impossible one. When countries whose currency is depreciated on so considerable a scale see their way to balancing their Budgets regularly they should consider means of giving the currency unit a gold value lower than its nominal gold equivalent.

Note.—Mr. A. H. Gibson dissents from this recommendation, and writes :—'France should eventually be able to bring the franc back to its old gold parity. Of all the schemes that could be suggested to lessen the penalties yet to be paid for inflation, devaluation of currencies is the most pernicious and inequitable. It would amount to confiscation of past savings of the working classes and of the fixed-income class to a relatively greater extent than of the savings of those who have made great fortunes since 1914. To put into operation a devaluation scheme would accentuate the many inequalities in sacrifice already caused by the recent War, and inflation, unless applied on the graduated scale system—people with small savings receiving a greater number of units of the new currency in exchange for a certain number of units of the old currency than in the case of people with greater wealth; but such a modified scheme would present practical difficulties to put into operation. What is wanted is the institution of a great international credit system, international control of exchanges, and the gradual letting down of the balloons of inflation over the next ten years.'

The Age of Stone Circles.—*Report of Committee* (Sir C. H. READ, *Chairman*; Mr. H. BALFOUR, *Secretary*; Dr. G. A. AUDEN, Professor Sir W. RIDGEWAY, Dr. J. G. GARSON, Sir ARTHUR EVANS, Sir W. BOYD DAWKINS, Professor J. L. MYRES, *and* Mr. H. PEAKE) *appointed to conduct explorations with the object of ascertaining the Age of Stone Circles. Drawn up by the Secretary.*

THE work of the Committee this year has been twofold. In the first place, the grant of 30*l*. from the British Association has been expended in filling in and making good the area excavated at Avebury in 1914 and during the present year. This reconstruction work has been duly carried out to the satisfaction of the owner, Mr. J. Peak-Garland. When the disturbed ground has completely settled down it will be necessary to do some final making good, but the small balance still in hand should cover the expenses involved. It is essential that this balance may be available for the purpose, as some time must elapse before the extent of the inevitable shrinkage is known.

In the second place, as there was a sum of money available from private subscriptions for purposes of renewed excavations on the site, the Committee decided to extend the excavations, begun in 1914, in the fosse to the east of the Kennet causeway, it being considered that this would be likely to yield the most profitable results, and that concentration upon this spot would have the advantage of facilitating and rendering less costly the filling in both of the old and the new excavations. The Committee, further, desired to measure the amount of silting accumulated in the fosse since 1914 in that portion which had been completely dug out in that year. It was thought that an idea could be gained as to the *rate* at which the original silting must have accumulated in the first few years of the monument's history. For reasons mentioned in Mr. Gray's detailed report, it was impossible to arrive at an accurate estimate on this point. At the same time, it was rendered evident that the chalk *talus* derived from the very steep sides of the fosse, freshly exposed in 1914, had accumulated very rapidly, and it can be inferred that objects dropped upon the original bottom-surface of the fosse must have been covered over and buried in the silting almost at once, and, consequently, that objects recovered from the lowest undisturbed layers of silt must be practically contemporaneous with the original formation of the fosse.

As in former years, Mr. H. St. G. Gray was employed by the Committee to act as overseer on their behalf, and to arrange for and carry out the work determined upon. Instructions were given to him as to the principal objectives, and he has carried out the operations in a very thorough and businesslike manner.

Incidentally, the excavations revealed a very pronounced variation in the level of the original bottom of the fosse in the area examined. This important feature, combined with other evidence, makes it clear that the fosse was not flooded, to serve as a moat, as has been suggested.

In spite of the regrettable paucity of important 'finds,' the net result of this year's excavations is to confirm the opinion arrived at from the Committee's former explorations at Avebury, and also at Arbor Low (1901-2) and the Stripple Stones (1906), and it may with increased confidence be urged that the great monument at Avebury was probably constructed during the later phases of the Neolithic period.

A considerable number of archæologists and others visited the excavations, which aroused much interest. The Secretary to the Committee stayed at Avebury for several days while the work was in progress.

It is hoped that the results of all the excavations undertaken by the Committee and carried out by Mr. Gray may eventually be brought together and published in book form, and that they may thus be made available to archæologists and others interested in our megalithic monuments. In view of this the Committee ask to be reappointed, but do not apply for a fresh grant.

Mr. Gray's detailed report upon this year's excavations is appended, and is a valuable addition to the material contained in the series of reports previously published between the years 1901 and 1915.

The Committee desire to thank Mr. Peak-Garland for permission to excavate on his property and for help kindly given by him, and also to thank Colonel L. D. C. Jenner for the loan of appliances.

THE AVEBURY EXCAVATIONS, 1922.

By H. St. George Gray.

I. Introductory Remarks.

After a long cessation of activities at Avebury, the excavations conducted by me under the direction of the 'Stone Circles Committee' of the British Association were continued and completed in the spring. The work had been in progress at intervals from 1908 to 1915.[1] This season our attention was for the most part concentrated upon the large fosse digging, Cutting IX, which had been partly examined in 1914 and then fenced in in the hope of completing the work in the following year. But the Great War intervened and rendered it necessary, in the interests of safety, to have the stake and wire fencing repaired on two occasions.

It was estimated that in the time at my disposal we might complete the re-excavation of the fosse in this position, and that before leaving I might arrange details for completely filling in this large cutting, not only with silting removed this season but also with the material which had been removed in 1914 and which now had become exceedingly compact and hard.

The work was begun on Monday, April 10, and the excavations were completed on Monday afternoon, April 24. A maximum number of thirteen men was employed; only three of them (including the foreman) had had previous experience at the Avebury excavations for one or more seasons. The weather was cold and windy, but little time was lost owing to heavy rain.

The filling-in was done by six men, who began on April 25 and completed the work to the satisfaction of Mr. Peak-Garland, the owner, on May 16.

Sectional diagrams and a plan of the fosse were made as the work proceeded, the position of the more important objects being indicated by numbers. Twelve satisfactory photographs (half-plate) were taken during the season, and these, added to those taken between 1908 and 1914 (which number 109), give a complete representation of the excavations and form a valuable photographic survey of Avebury.

On our arrival we found the material thrown out in 1914 much overgrown with young trees which had seeded themselves. While these were being grubbed out I measured the amount of talus which had formed in the partly re-excavated fosse during the time which had elapsed since May 1914. In that year we had exposed a length of only 4.25 ft. of the original floor of the fosse, and for a length of another 5 or 6 ft. the re-excavation had reached to within 2 or 3 ft. of the bottom. The talus *appeared* to be very considerable, and in the middle of the cutting its surface was only 13 ft. below the surface of the silting as found at the beginning of the excavations in 1914, and about 12 ft. above the floor of the fosse. Nearly two-thirds of the end-wall forming the causeway had become covered by loose material, mostly chalk, in the eight years.

But the value of these calculations was greatly lessened when, on clearing out this part of the fosse again and instituting inquiries in the village to supplement what was obvious to the eye, it was found that several tons of rubbish, tins, crockery and bottles, as well as some stone, had, at intervals during the eight years, been shot into this deep hole, which apparently formed a decided attraction to those in this village and elsewhere who had rubbish to dispose of.

[1] The reports already published giving details of the excavations are as follows: *Brit. Assoc. Reports*, 1908, pp. 400-413; 1909, pp. 271-284; 1911, pp. 141-152; and 1915, pp. 174-189.

II. General Observations on Cutting IX, through the S.S.E. Fosse, 1922.

The fosse excavation marked out for examination in 1914 was slightly extended to 44.5 ft. in length, and the width was regulated by the line taken by the escarp and counterscarp of the fosse, after the fosse had slightly increased in width owing to exposure of the upper parts to the atmosphere since 1914. The width at the east end was, however, for sectional purposes, put at 35.5 ft.

It took the greater part of the first week to remove the rubbish and the eight years' accumulation of silting; and from that time the previously untouched silting, consisting of mould at the top, mixed silting lower, and chalk rubble in the bottom half, was removed in such a way that it passed through three or four hands, thereby lessening, even with partly untrained men, the chance of missing pottery, flints, bones, and other remains mixed with the silting. 'Finds,' however, of small kinds were few, and were it not for the red-deer antler (picks, levers, &c.) the number of the relics would be decidedly small.

The Report of 1915 should be read in conjunction with the one now presented, for little that was said there about Cutting IX, Fosse, is repeated here.

Structurally, this part of the fosse was most interesting, not only by reason of its enormous depth and size but also on account of the great irregularity of the bottom. Then, with regard to the 'finds,' negative evidence was again afforded by the entire absence of any trace of metal in the lower silting (chalk rubble) of the fosse. Near the bottom of the mixed silting and above the chalk rubble, fragments of pottery of the beaker type were found in these excavations for the first time.

Depth of the Fosse.—At the close of the excavations of 1914 I could measure the depth of this part of the fosse approximately only, owing to bad weather and difficulty in getting a true vertical measurement. The depth from the brink of the fosse to the original floor at the west end of the cutting was given as 29.5 ft. (see *B.A. Report*, 1915, p. 178). This should be increased by 0.75 ft., making the maximum depth of the fosse at this point 30.25 ft.[2] The vertical measurement from the top of the ancient causeway to the bottom of the fosse proved to be 35.75 ft., and from the crest of the vallum to the floor about 55.5 ft. (This is the only statement I have to correct in the 1915 Report.)

The Silting.—On removing the silting at the east end of the cutting it was found that in the middle the top of the chalk rubble was reached at a depth of 7 ft., but owing to the curvature of the strata in the silting the chalk rubble on the sides of the fosse extended almost to the top. This concavity in the layers of the silting was extremely well seen on the east face of this cutting just before the completion of the digging and during the final stages in clearing up the floor. It was intended to photograph this interesting feature on the last day of the work, and the silting was being specially trimmed down for the purpose. However, owing to wind, hail, and rain this loose face of silting would not stand, and a few tons of the material crashed to the bottom of the fosse, smashing our largest ladder in the fall. This disaster was much to be regretted, as this 'face' showed excellently the strata representing the long periods of time during which the silting had formed.

The upper $2\frac{3}{4}$ ft. of silting consisted of mould containing modern and mediæval remains. Roman objects extended down to a maximum depth of 3.8 ft. Everything below that (in the middle) was of prehistoric date. At a maximum distance of 10 ft. from the east end of the cutting several pieces of sarsen stone were found at depths varying from 5 ft. to 5.6 ft. They measured from 0.5 ft. to 0.75 ft. across, and were apparently not arranged in any special order. Much fewer and smaller fragments were found between 3.5 ft. and 5 ft.

In clearing the bottom of the fosse it was found that the chalk rubble in the middle and near the causeway was very large, some pieces measuring 0.75 ft. across. At the sides of the fosse, as would be expected, the rubble was much smaller. Small pieces of ochreous clay were occasionally met with in the rubble.

Charcoal.—A piece of charcoal found deep in the rubble proved to be Hazel (*Corylus Avellana*, L.). At the bottom of the yellowish-brown mould, depth 6.8 ft., charcoal was collected, which Mr. A. H. Lyell also examined; this was identified as Hornbeam (*Carpinus Betulus*, L.).

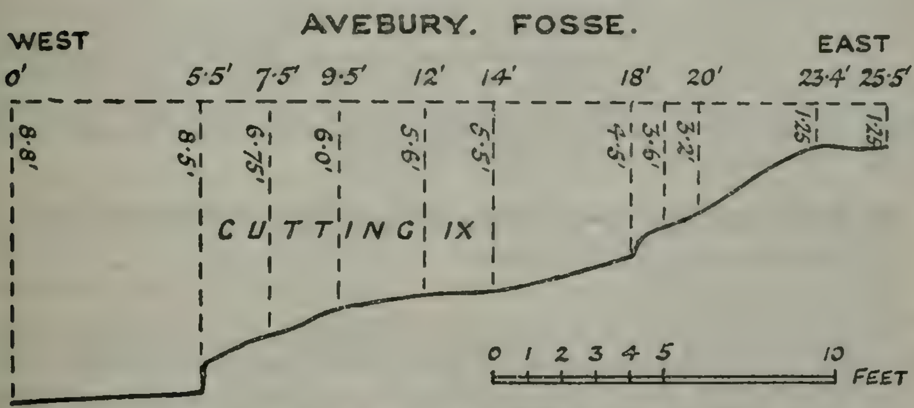
[2] Taken along the sloping chalk wall of the fosse from brink to floor the measurement was 34 ft.

Animal Bones.—Animal bones were occasionally met with, but nothing of any special interest. In the Roman layer a metacarpus of ox gave a height at shoulder of 3 ft. 5½ in., and in the mould at a depth of 4.8 ft. another was found which gave a height of 3 ft. 4¼ in. The modern Kerry cow averages 3 ft. 5 in. at shoulder. A dog's jaw was also found in the Roman stratum, and another of a smaller dog in the lower part of the chalk rubble; and near the top of the chalk rubble in the middle of the fosse the greater part of a goat's skull (young).

A radius of horse was found deep in the fosse; also part of a rib of ox or horse, smooth and scratched (but not cut), close to the bottom of the fosse.

Bottom of the Fosse.—As mentioned in the previous report (1915), the steepest part of the fosse-wall revealed during the excavations was at the west end of this cutting where the wall represents the east face of the solid entrance-causeway. The greatest steepness was in the lower 8 or 10 ft., not only at the end but also at the north and south sides immediately adjacent to and bounding the deepest part of the fosse.

From the end-wall eastward it was found that the floor previously uncovered in 1914 for a length of 4.25 ft. was practically level right across the fosse, the width of which in this part was 14 ft. At 5.5 ft. from the end-wall there was an abrupt rise of a foot; and, as the accompanying sectional diagram shows,



SECTION SHOWING A RISE OF 7·55 FT. IN THE FLOOR OF THE FOSSE IN A LENGTH OF 25·5 FT. H.St.G.G.

taken along the middle of the fosse, the floor continued to rise up to the limit of our digging eastward, and it would have been interesting, had time and funds permitted, to continue the re-excavation of the silting still further eastward, if only to ascertain what variation would take place in the relative depth of the floor of the fosse. In the length we were able to expose, viz. 25.5 ft., the floor rose from west to east to the extent of no less than 7.55 ft., and at the east end the bottom had diminished to a width of 8 ft. Along the north side of the floor, in the middle 9.5 ft., there was a decided concavity suggesting a rough pathway. Towards the west it approached a rather deep recess in the north-west corner of the fosse-wall, which extended practically to the top of the fosse. This recess or 'shute' might have been caused by wear in hauling up baskets of loosened chalk by means of ropes in the original formation of the fosse. A similar recess, but much less marked, was noticeable in the south-west corner (*B.A. Report*, 1915, p. 178). If a large amount of the chalk excavated in the construction of the fosse was, in this part, brought to the north-west and south-west corners of the fosse, this would probably account to a large extent for the worn depression or 'pathway' mentioned above. At from 8 ft. to 13.5 ft. from the end-wall there was on the north side a natural vertical and smooth face of solid chalk reaching to a maximum height of 3 ft.

III. The Moat Theory.

In the fosse excavations before the present season we found the floor fairly smooth, except in Cutting VIII (*B.A. Report*, 1911, p.144), where in a length of 21.5 ft. the levels varied to the extent of 2.95 ft.

Having found the floor so irregular in places, especially in the last cutting made, and having more than once heard the suggestion made by antiquaries visiting the excavations that the Avebury fosse might have been a moat supplied by the waters of the River Kennet, I was particularly interested at the time of the excavations to read Mr. A. D. Passmore's short article, entitled 'The Avebury Ditch,' in *The Antiquaries' Journal*, II., 109-111, which was written without any reference or inquiry with regard to my surveys at Avebury. The writer speaks of a ditch being planned with a level bottom 'irrespective of the original level of the ground at any one point'; and he also argues that 'a level of 10 ft. of water could be maintained in the moat surrounding Avebury Circles.'

During my stay at Avebury I brought my levels into correlation with those given on the 6-in. Ordnance Sheets, and those recorded by Mr. A. H. Lawson in the article referred to. Before doing this I was fully aware from my sectional diagrams that the levels of the bottom of the fosse in the different places I had excavated on the S.S.W. and S.S.E. varied considerably, and that the floor of the fosse rose from west to east, so far as my five cuttings are concerned, to the extent of 11.6 ft. The lowest point, on the west, is not much below the present bed of the Kennet stream (according to its level given in *The Antiq. Journ.*). I hope to work out the details on another occasion, but it was pretty obvious to those who saw the floor of Cutting IX, Fosse, exposed this season, with its rise of 7.55 ft., that the great ditch could not have contained water. Moreover, in none of the cuttings made had any sediment or staining of the 'walls' been noticed in the lower parts of the fosse.

IV. Finds from the Fosse, Cutting IX (excluding Antler-picks, &c.).

(*a*) *Remains from Roman Stratum.*

256. Five fragments of black and brown Romano-British pottery, including a rim piece. Depth 3.5 ft.

269. Bronze ring, of plano-convex cross-section; ext. diam. 20 mm.; perhaps a small finger-ring. Depth 3.8 ft. (It has the appearance of being of late date, and may have worked its way down through a hole.)

270. Three fragments of Romano-British pottery, including a rim piece. Scattered, at depth of 3.4 ft.

(*b*) *Human Remains.*

278. A few human bones, consisting of a fragment of cranium, piece of a lower jaw and fragments of bones of a fore-arm. Found at the E. end of the cutting, depth 6.3 ft., in the lower foot of the yellowish-brown mould. (In this light-coloured mould a large number of recent shells were observed.)

(*c*) *Flint.*

257. Large scraper, of rough workmanship, 2¼ in. by 2⅛ in. Found in the yellowish-brown mould, actual depth unknown, but not exceeding 7 ft. below the surface of the silting.

258. Implement, perhaps a scraper, having a prominent bulb of percussion. Depth 5 ft., in the yellowish-brown mould.

A calcined flint (small), found deep in the chalk rubble.

V. Prehistoric Pottery.

279. This, the last 'find' made in the excavations, is in some ways the most important, as it is the only occasion on which we have found fragments of the beaker or drinking-vessel type of prehistoric pottery in the fosse. There are over a dozen unmistakable but small fragments (the largest 1 in. across) of this ware, with typical notched patterns in chevrons, horizontal and vertical lines, and with plain zones dividing the bands of ornament. The ware of fine paste is for the most part reddish-brown on the outside and brown on the interior surface, and it is probable that it belongs to more than one vessel. Some of the pieces have scaled, but those that retain their full proportions vary

from 5 to 7.5 mm. in thickness. With the fragments, which were scattered over an area measuring some 15 in. across, was found a well-struck flint flake.

The pottery was found [3] in the middle of the silting of the fosse on the east margin of the cutting in burnt earth, nearly at the bottom of the mixed silting and 6.5 ft. below the surface, and barely 0.5 ft. above the top of the white chalk rubble.

Prehistoric pottery of the West Kennet and Peterborough type has already been found in appreciable quantity at the bottom of the mixed silting in the Avebury fosse at a level corresponding exactly with the beaker fragments found this season. There is probably little difference in the date of these wares, and although the vessels with round bottoms, such as have been found at Peterborough, Mortlake, &c., are probably older as a type, it is now regarded as proved that the manufacture of this class of pottery overlapped to some extent with the earliest types of beaker pottery. The association of beakers with typical Neolithic ware, as Mr. Reginald Smith has pointed out, 'is all in favour of the introduction of the beaker into this country before bronze was in use on this side of the North Sea, though metal had become fairly common before the beaker passed out of fashion.' [4]

It will be appropriate and interesting to recall here the discovery made by Mr. and Mrs. B. H. Cunnington of a human skeleton with fragments of a beaker close to and immediately in front of the hole in which the stone 'Adam' stood at the western end of the Beckhampton Avenue, in the parish of Avebury. 'Adam' fell on December 2, 1911, and it was during the preliminaries preceding the re-erection of the stone that the discovery was made. It was evident that this important interment was in its original position.[5]

VI. Picks and other Remains of Red-Deer Antler, found in the Fosse (Cutting IX).

Picks or red-deer antler and other tools of the same material were again fairly plentiful in this cutting. Twenty numbered specimens were found in this part of the fosse in 1914, to which we are now able to add seventeen as follows. (All the picks are formed from shed antlers; all were found in the chalk rubble.)

259. Crown of an antler, one of the four points missing, another broken off at the tip; the two remaining points much worn down by use.

Found in the S. half of the cutting near the W. end: depth 27·5 ft. below the brink of the fosse on the E. side of the causeway. (The depths in this section of the report are all given from the same point.)

260. Worked tine of red-deer, bevelled and smoothed at the tip.

Found in the N. half of the cutting, depth 20 ft.

261. Greater part of the beam of an antler, the stumps of the intentionally removed bez- and trez-tines remaining. The base of the antler and the brow-tine were not found; they were evidently broken off owing to prolonged use of this pick as a hammer also (the specimen shows considerable evidence of hammering).

Found in the N. half of the cutting near the middle of the silting, depth 21 ft.

262. Worked tine, broken at the tip.

Depth 27.3 ft.

263. Pick, small but well worn, consisting of the beam and burr of a shed antler. The bez- and trez-tines are reduced to stumps, and the brow-tine is partly broken off. The back of the base of the antler and the burr bear clear evidence of wear.

Found in the middle of the cutting, depth 27.5 ft.

264. Antler of young animal, feebly developed, having all its four points remaining (including the brow-tine); they are smooth and show traces of human work. Most of the burr has been removed, apparently by hammering.

Found near No. 263, and at the same depth.

[3] I collected the fragments myself, using a small trowel for the purpose.

[4] *Archaeologia*, LXII., 351. A part of the Avebury Report for 1911 should also be read in this connection (*B.A. Report*, 1911, pp. 150-151); and Mr. Leeds' paper on 'Further Discoveries at Peterborough' (*The Antiquaries' Journ.*, II., 220-237).

[5] *Wilts Arch. Mag.*, XXXVIII., 1-7.

265. Pick in fairly good condition, having the brow-tine remaining and the bez- and trez-tines reduced to stumps. The brow-tine has become shortened by wear and its 'tip' is now quite blunt. The handle-end bears traces of fire. Length 442mm. (17⅜ in.).

Found in the middle of the cutting on the bottom of the fosse, depth 28.5 ft.

266. Pick of large dimensions and the finest found this season; length from burr to end of handle 503 mm. (19⅞ in.). The brow-tine, smooth at the tip, is of graceful form, length 340 mm. (13⅜ in.) on the outer curve. The bez- and trez-tines have been considerably cut down. The smoothed handle bears indications of fire in two places. Min. circumference of beam between bez- and trez-tines, 153 mm.; circumference just above the burr, 200 mm.

Found in the N. half of the cutting near the W. end, on the bottom of the fosse, depth 29.5 ft.

267. Crown of an antler, consisting of one large and two smaller points; one of the latter is remarkably smooth.

Found near No. 266, depth 29 ft.

268. Crown of a large antler, having all three points 'worked.'

Found in the N. half of the fosse, depth 27 ft.

271. Pick, having the burr completely worn away and the brow-tine largely reduced by constant use. One tine has been completely removed; the beam of the antler is smoother than is generally the case. The crown of two points has been left to form a termination to the handle of the pick; these points are also a good deal polished by wear. Length 522 mm. (20½ in.).

Found near the middle of the cutting, on the bottom of the fosse, depth 24 ft.

272. Crown of a large antler, having two of the three points remaining; but only one is nearly complete and smooth at the end.

Found on the bottom of the fosse.

273. Pick, small, but fairly complete, except at the handle-end. The brow-tine has, however, been shortened by fracture.

Found at the bottom of the fosse, depth 24 ft.

274. Pick, small and incomplete, with the whole of the brow-tine deficient. Part of the burr has been removed.

Found near the bottom of the fosse, depth 19.5 ft.

275. Beam of a large antler, without any trace of the burr or brow-tine now remaining. The handle-end is extremely smooth—more polished than the great majority of the Avebury specimens. Probably the remains of a pick.

Found within 3 in. of the bottom of the fosse, in the N. half of the cutting.

276. Greater part of a pick, having the brow-tine set very obtusely to the line of the beam of the antler. The bez- and trez-tines have been removed; also part of the burr; handle-end broken.

Found near the E. end of the cutting, depth 22 ft.

277. Pick; with the exception of No. 266 the finest specimen found this season. The brow-tine is worn down to a rounded point, the result of considerable use. The bez- and trez-tines have been removed. The handle-end shows some signs of wear. The most pronounced indication of wear, however, is seen at the back of the beam and round the burr, caused by hammering. Length 431 mm. (17 in.).

Found near the E. end of the cutting, depth 19.5 ft.

VII. Discovery of Buried Stones in the Northern Inner Circle.

For some years past I have known of part of a sarsen stone showing one or two inches above the surface in the farmyard near 'The Cove.' This year Sir Prior Goldney and Mr. A. F. Major also noticed the slightly protruding stone, and probing revealed the fact that it extended for some distance. Expecting that this stone might be of sufficient importance to mark on my plan, I had this area dug over by two men. The stone proved to have been buried by penetrating the solid chalk for the purpose. In plan it measured 5.8 ft. in length by 4.5 ft. in width at the west end; at the east end it was 3.5 ft. wide; it was a thick stone of almost quadrangular cross-section; its thickness at the west end appeared to be 1.7 ft.

Extending the digging slightly both north and south, two other buried stones were uncovered, the most southern barely reaching the present surface at its west end. The most northern stone of the three was only 5 in. deep below the surface at the west end; at the east end it was rather deeper. It was found that the three stones (now numbered XXVII, XXVIII, and XXIX from north to south) covered a length of 16.5 ft. No. XXIX was a triangular stone 7.3 ft. in length, with a maximum width at the west end of 6.2 ft., where the

thickness of the block appeared to be 1.25 ft. The most northern stone. No. XXVII, was of more or less triangular shape, having a maximum length of 7.5 ft. and a maximum width of 4.15 ft.; thickness about 1.75 ft. Nos. XXVII and XXVIII were only 0.35 ft., and Nos. XXVIII and XXIX 0.75 ft. apart. Nos. XXVII and XXIX were lying fairly flat, but No. XXVIII sloped eastwards at an angle of some 45°.

These stones, which have been covered up again at the wish of the owner, Mr. Peak-Garland, are to the east of the two great standing stones, known as 'The Cove.' The nearest parts of Stones XXVII and XXVIII are about 29 ft. east of the highest stone of 'The Cove' (No. XXVI).

VIII. Concluding Remarks.

There can be little doubt that Avebury—its circles, fosse and vallum—is referable to the late Neolithic period. This belief has been sustained by the evidence covered by five seasons of excavation. The total absence of metals in the lower parts of the silting of the fosse and in the vallum cutting affords strong negative evidence. The persistence of tools of stone, antler and bone, including flint implements, antler picks, hammers, rakes and levers, bone shovels and other worked bones, at least strongly suggests Neolithic date.

The evidence, too, of Neolithic date is greatly strengthened by the discovery of a representative collection of fragments of prehistoric pottery, some of types which have yet to be compared more closely with similar pottery in the light of recent discoveries elsewhere. When the Avebury excavations began in 1908 comparatively little was known as to the details which afforded a clue in dividing Neolithic from early Bronze Age pottery with any degree of certainty.

We have not been richly rewarded in the way of relics, but considering the early date and great extent of Avebury we might well have found less. The excavations have been worth while even if there had not been any other purpose in view than to ascertain the method of construction of the great fosse—its vast proportions, enormous depth, irregularity in width at bottom, and uneven floor. Incidentally, much else has been done. We have a complete scale plan and some sections of the monument, and a photographic survey has been carried out. The ancient entrance-causeway on the south has been located and partly exposed by excavation. The socket-hole of one prostrate stone has been cleared, with interesting results, and three buried stones have been rediscovered in the northern inner circle.

Had it been possible in this particular exploration, it would have been interesting to ascertain if there is an entrance-causeway on the north (Swindon road), and also whether there is a western entrance. The latter would be exceedingly difficult to prove by excavation, as the probable site is on the line of the village street.

The Distribution of Bronze Age Implements.—*Interim Report of Committee* (Professor J. L. Myres, *Chairman*; Mr. Harold Peake, *Secretary*: Dr. E. C. R. Armstrong, Dr. G. A. Auden, Mr. H. Balfour, Mr. L. H. D. Buxton, Mr. O. G. S. Crawford Sir W. Boyd Dawkins, Professor H. J. Fleure, Mr. G. A. Garfitt, Dr. R. R. Marett, Mr. R. Mond, Sir C. H. Read, Sir W. Ridgeway).

The Committee has had throughout the assistance of Dr. H. S. Harrison, representing the Royal Anthropological Institute, and Lord Abercromby, representing the Society of Antiquaries of Scotland.

The Committee's draughtsman, Mr. C. H. Howell, has been employed throughout the year, and has paid several visits to museums and private collections within the Home Counties; Mr. C. O. Waterhouse has drawn about 100 specimens in the British Museum.

Again we have received help from volunteers, especially from Curators of Museums, notably those of Ipswich, Norwich, and Cardiff. Mr. E. C. Middleton has continued

his work among the museums and private collections in the Midland counties, and Mr. Wallis has drawn on cards all the specimens in the Royal Scots Museum. The work of copying on to cards the original drawings of Mr. Armstrong's catalogue of gold ornaments is all but completed.

The number of cards completed exceeds 6,000, and a rough idea can be formed of the work outstanding; it is estimated that this is:

England . . .	2,500 to 3,000
Scotland . . .	800 to 1,000
Total . . .	3,300 to 4,000

It is impossible at present to make any estimate for Ireland.

If the services of sufficient volunteers can be obtained, the cost of completing the work in Great Britain would not much exceed 200*l*., but it seems unlikely that this will be the case, and it seems probable that from 300*l*. to 500*l*. will be required.

The following sums have been received:—

	£	s.	d.
The British Association	50	0	0
Do. do.	50	0	0
The Society of Antiquaries . . .	20	0	0
Lord Abercromby	10	0	0
G. A. Garfitt, Esq.	10	0	0
Parker Brewis, Esq.	10	0	0
R. Vernon Favell, Esq.	5	0	0
H. R. Beeton, Esq.	2	2	0
George Cadbury, junr., Esq. . . .	2	0	0
Christopher Martin, Esq.	1	1	0
Lt.-Col. W. Ll. Morgan	1	1	0
Dr. A. Corner	1	1	0
James Booth, Esq.	1	1	0
C. E. Keyser, Esq.	1	1	0
H. Gordon Selfridge, Esq. . . .	1	1	0
Lord Leverhulme	1	0	0
	166	8	0
Balance from last year . . .	63	14	4
Total receipts	£230	2	4

The expenditure has been mainly on draughtsmen:—

	£	s.	d.	£	s.	d.
C. H. Howell, salary	153	0	0			
C. O. Waterhouse, at piece rates . . .	7	13	0			
				160	13	0
C. H. Howell, expenses	19	18	6			
E. C. Middleton, do.	0	18	0			
				20	16	6
Boxes and cards				5	13	0
Cheque-book and bank charges . . .				0	11	8
				£187	14	2

Summary.

	£	s.	d.
Total receipts	230	2	4
Total expenditure	187	14	2
Balance, June 30, 1922 . . .	£42	8	2

Lake Villages near Glastonbury.—*Report of Committee* (Sir W. BOYD DAWKINS, *Chairman*; Mr. A. BULLEID, *Secretary*; Mr. H. BALFOUR, Mr. WILLOUGHBY GARDNER, Mr. F. S. PALMER, Mr. H. J. E. PEAKE) *appointed to investigate the Lake Villages in the neighbourhood of Glastonbury in connection with a Committee of the Somerset Archæological and Natural History Society.*

THE Committee for exploring the Lake Villages in Somerset beg to report that after a lapse of seven years the excavations at Meare were reopened on Monday, August 29, 1921, under the direction of Messrs. A. Bulleid and H. St. George Gray, and were continued for three weeks.

The Meare Lake Village consists of two distinct groups of mounds extending over portions of six pasture fields. Up to the time when war broke out the excavations had been restricted to the mounds in one of the fields of the western group, and some twenty dwellings had been explored, together with the ground around and between them.

Last year work was carried on in the easternmost field of the west group, and also a small portion of ground on which the directors' shed stood in the old excavation field part of Mound IX.

The field in which the excavations are now being carried on belongs to the Somerset County Council, who have given every facility for the work, and the Committee wish to express their heartiest thanks for the help they have received from that body.

Portions of the following dwelling mounds were examined : *i.e.* Mounds IX, XXI, XXIII, XXIV, and XXXVIII.

The usual structural arrangement of timber for the foundation underlying the clay floors was met with, but so far no additional information was obtained regarding the size, shape and construction of the dwellings. As yet no palisading has been discovered surrounding the village, nor a causeway leading to the high land.

Among the smaller objects of interest discovered the following may be mentioned.

Bronze.—Miniature chopping-axe with socket-hole, an amulet or toy (the first of the kind found in the Lake Villages). Two miniature axes with handles (all of bronze) were found by General Pitt-Rivers in Bokerley Dyke. Others have been found at Silchester.

Fibula of La Tène III type; and another of the first half of 1st century, A.D.

Two bars, or rods, of a kind not found previously.

Child's finger-ring of one-and-a-half coils; and a full-sized spiral finger-ring.

Several rivet-heads, similar to those which ornament the bronze bowl found at the Glastonbury Lake Village.

Tin.—Wheel-shaped ornament, or amulet, precisely similar to one found in the same dwelling (No. IX) in 1914. Two lumps of tin ore.

Iron.—Fragments apparently of two spear-heads, small cold-chisel, &c. Also a long bar, with one end considerably expanded and flat, length 26½ inches. We know of no similar objects except the five undescribed specimens found at Hunsbury (*Assoc. Architect. Socs.*, XVIII, Pl. vii, figs. 3, 4).

Crucibles.—Parts of three, including the greater part of the largest crucible found in the village.

Glass.—Several globular beads and ring-beads; mostly of opaque yellow paste. One specimen is of dull purple glass. Others are of clear glass with streakings of lemon and orange colours.

Kimmeridge Shale.—A large number of 'finds' of shale were made, including half an armlet (Halstatt type), ornamented, a plain armlet (in halves), and parts of two armlets with rivet-holes for repair at both ends.

Antler.—Amongst the many worked pieces of antler are the following : Four weaving-combs, two of them in excellent condition ; cheek-pieces (mostly broken) ; knife-handles ; a heavy pin, much worn ; a pin with an expanded end, for personal decoration (perhaps used as a hair-pin) ; a number of short strips or lengths of antler, shaped by means of a knife (precise use unknown).

Bone.—A 'toggle,' or dress-fastener; two polishing-bones; modelling-tool; two tibiæ, with perforations and sawn notches; a highly polished length of bone of quadrangular cross-section. Perforated boar's tusk. Large number of animal remains.

Pottery.—In some quantity, including a large number of parts of pots finely ornamented; two pieces of pot covers; fragments with perforated bases; and the pedestalled base of a vase.

Baked Clay.—Triangular loom-weight; fragment in the form of the end of a model boat, with two perforations.

Spindlewhorls.—Several of stone, pottery, baked clay, and bone, including an ornamented specimen (very few ornamented examples found previously).

Flint.—A barbed and tanged arrowhead, several scrapers, and small pieces of worked flint.

Querns.—Stones of saddle querns.

Derbyshire Caves.—*Report of Committee* (Sir W. Boyd Dawkins, *Chairman*; Mr. G. A. Garfitt, *Secretary*; Mr. Leslie Armstrong, Mr. E. N. Fallaize, Dr. R. R. Marett, Mr. H. Peake, Prof. W. M. Tattersall) *appointed to co-operate with a Committee of the Royal Anthropological Institute in the Exploration of Caves in the Derbyshire district.*

During the season 1921-22, members of the Committee have conducted or supervised explorations in Derbyshire caves at Cresswell Crags, Gressbrook Dale (especially at Ravencliffe Cave), Longcliffe Crags (Harborough Cave), near Brassington, four caves in Hartle Dale, near Castleton, and the 'Demon's Dale' in the Taddington area. Arrangements have been made for further excavation of this cave, where only the upper layers have been examined hitherto. For a preliminary report by Mr. Storrs Fox see *Proc. Soc. Ant. Lond.*, *II*, xxii, 129 (1908). A chance discovery of human and animal remains at Castleton led to the examination of a collapsed cave by Messrs. L. Armstrong and R. V. Favell. A fuller report of this year's work will be published in *Man.*

Experimental Studies in the Physiology of Heredity.—*Final Report of Committee* (Dr. F. F. Blackman, *Chairman*; Miss E. R. Saunders, *Secretary*; Professor W. Bateson, Professor Sir F. Keeble).

During the past year the work on the surface anatomy of the higher plants which was begun in 1921, and briefly reported at the Edinburgh meeting of the Association, has been continued and extended. Examination of the seedlings of various species employed in certain breeding experiments and of other material has yielded evidence supporting the view put forward originally by Hofmeister and Naegeli that the stem must be regarded as consisting of an axial core surrounded by a foliar skin. A full account of the observations and the deductions therefrom appears in the April issue of the *Annals of Botany.*

Further breeding experiments have been carried out on *Matthiola* with the object of ascertaining whether other linkages, in addition to those already described, exist between the several factors which have now been identified, but these results are not yet complete. It is proposed to continue the work, and it is hoped that sufficient financial assistance from another source will be available in the coming year to enable this to be done. The Committee do not therefore seek reappointment.

Training in Citizenship.—*Third Report of Committee* (Right Rev. Bishop WELLDON, D.D., *Chairman*; Lady SHAW, *Secretary*; Lieut.-Gen. Sir ROBERT BADEN-POWELL, K.C.V.O., K.C.B., Mr. C. H. BLAKISTON, Mr. G. D. DUNKERLEY, Mr. W. D. EGGAR, Mr. J. C. MAXWELL GARNETT, C.B.E., Sir RICHARD GREGORY, Mr. SPURLEY HEY, Miss E. P. HUGHES, LL.D., Sir THEODORE MORISON).

Report.

THE work of the Committee during the current year has been divided into three parts :—

1. The printing and distribution of the reports presented in 1920 at Cardiff and in 1921 at Edinburgh.
2. Compilation of a Bibliography of Civics.
3. A survey of the training methods adopted in the schools of the Empire for the formation of character.

The Committee desire to thank the Association for the additional number of 500 of the Cardiff and 250 of the Edinburgh reports, and for the permission to use the type of the reports to obtain any further copies that might be deemed desirable.

In September 1921, with some help from friends, 2,000 copies of the Cardiff report were obtained, and in December 1921 the Committee purchased 4,000 copies of the Cardiff and 250 copies of the Edinburgh report. Copies have been supplied to all the Education Authorities, and, through the Board of Education, the Scottish Education Department, and the Irish National Board of Education, to all the Inspectors of schools in the United Kingdom; 4,965 of the Cardiff reports and the whole of the Edinburgh reports have been circulated, the greater number by sales to the Education Authorities and persons engaged in, or interested in, education.

A remarkable and active interest has been evinced in the subject, and a large number of books have passed through the press.

The demand for trustworthy text-books which reached the Committee from teachers has led to the compilation of the Bibliography that forms the Appendix to this report. The Bibliography was in the first place drafted at the London School of Economics under the superintendence of the Librarian, Mr. Headicar, to whom the Committee's cordial thanks are due. To include in the list all books bearing on the subject was felt to be not only impossible but undesirable. Teachers for the most part have ready access to books on history, literature, philosophy, &c.; but civics is not yet recognised as a school subject. What is required is a knowledge of where the facts of civics as they exist at the present day can be found.

A knowledge of civics, the forms of central and local government, and of the laws under which we are governed and the provision made for the maintenance of the citizen in health, peace and freedom, is only a part of the training in citizenship. The good citizen will certainly seek for knowledge of such subjects; but such knowledge alone will not make a good citizen. It is a valuable part of his training, but only a help to the formation of his character.

In the course of inquiry the Committee have received a good deal of information upon the steps taken in various countries to supplement what may be called the secular side of civics by training the young of both sexes in the duties and virtues of good citizenship. There was published some time ago in New Zealand a remarkable Broad Sheet containing a great number of opinions offered by influential teachers and writers in many parts of the Empire upon the value of the Bible in the formation of character. The Committee, while they do not wish to raise the religious question by quoting any expressions from this Broad Sheet, yet feel justified in quoting three remarkable passages from the report of the Departmental Committee, over which Sir Henry Newbolt presided, upon the position of English in the educational system of England.

1. 'Where foreign writers cannot be studied in their works as they wrote them the motto of the student should be "not text-books but translations,"

or "not text-books until after translations have been read." Especially the greatest translation in English, perhaps in any language, should be universally read, far more read than it is at present both in universities and schools. We refer, of course, to the Authorised Version of the Bible, which is among the greatest of English classics, and has been the most influential of them all as well on English literature as on English life.' (End of par. 199.)

2. 'We have three plain facts before us. First, the Authorised Version, though a translation from an Eastern original, is a true part of English literature—has, indeed, been fitly described as "the most majestic thing in our literature and the most spiritually living thing we inherit." Second, it is historically true that for five centuries and more no other English book has been so widely read in this island, or so closely connected with our national life, or has left so strong a mark upon the mass of our literature. Third, at the present time the Bible is probably less widely read and less directly influential in our life and literature than it has been at any time since the Reformation. On such premises as these it might seem easy to base a recommendation.' (End of par. 310.)

3. 'The power of the Bible upon our language, our literature, our national life and thought, has been lost sight of because the possibility has not hitherto been imagined that a liberal education may be, and should be, not only a gift within the reach of every child, but the very gift purposed by the State in undertaking the elementary training of its citizens. From the moment when this is admitted it will seem to be no longer possible to deprive our schools of the free and impartial study of the Bible. If we set aside, as we do with any other classic, all consideration of its bearing upon dogmatic religion, there can be no division of opinion as to its historical position and effect in this country.' (Par. 311.)

The Committee wish to express their hearty agreement with the conclusion so stated, but it does not lie within their province to indicate the means by which effect should be given to the conclusion, whether in secondary or in elementary schools.

Appendix.

The Bibliography.

A Select List of References on Citizenship (Civics): mainly from English and American sources.

In including any book in the Bibliography the Committee must not be understood as adopting the views of the author.

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SECTIONAL TRANSACTIONS.

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE.

Thursday, September 7.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 408.)

1. Discussion on *The Origin of Magnetism.* Opener, Prof. P. Weiss (see p. 411).

2. Prof. Sir J. Larmor, F.R.S.—*On the Structural Significance of Optical Rotatory Quality.*

It is known that there are crystals, such as quartz, which exhibit high optical rotation, whereas the same substance in amorphous (fused) condition is devoid of it. It must then be due to chirality (right and left handedness) in the crystalline forms which is not present optically in the constituent molecule. A molecule may thus be chiral in geometrical form without being chiral in optical structure. The manner in which crystalline form alone can thus be effective for optical rotation has been analysed (*Roy. Soc. Proc.* 99A, Jan. 1921, pp. 7-11) on the hypothesis that the crystalline unit is the charged ion, not the bipolar molecule. This view of crystal structure was originated and confirmed over a wide range by X-ray ultra-optical analysis in the hands of W. H. and W. L. Bragg and others. The point now made is that it suffices to account for the optical rotation of substances such as quartz, which otherwise it would seem hardly possible to understand.

It is explained (*loc. cit.* p. 7) that owing to the screw structure as regards the ionic crystal elements, an impressed electric field produces a slight magnetisation which is a linear function of its time-gradient: and that is the efficient cause of the optical rotation. For isotropic screw quality the two vectors would be proportional, say $A = k \,.\, dP/dt$: and then the optical rotation per unit depth for radiation of period $2\pi/p$ would be $\pi p^2 k$. The optical rotation is, however, dispersive: therefore the ionic twist constituting the magnetisation A involves inertia as well as structural elastic reaction, and thus it has a period of free oscillation. The relation $A = k \,.\, dP/dt$ is thus to be replaced, to include variety of periods of the light, by one of type

$$a\frac{d^2A}{dt^2} + bA = \frac{dP}{dt}.$$

The previous form therefore still holds if

$$1/k = a\frac{d^2}{dt^2} + b = -ap^2 + b$$

instead of k being constant. The rotatory power thus assumes the form $\dfrac{\pi p^2}{-ap^2 + b}$, which is equivalent to $f(\lambda^2 - g)$, the free period of crystalline ionic twist being $c^{-1}\sqrt{g}$. For aeolotropy there may be more than one such period, up to three: the discussion may follow concisely the lines of 'Æther and Matter,' p. 356. In the Bakerian Lecture for 1921, Prof. T. M. Lowry has adduced evidence that for various substances one such term adequately represents the course of the dispersion.

This considers only optical rotation of crystalline origin. But in most substances part of the rotation persists in the amorphous state and in solution: therefore the molecule is itself chiral in its ionic structure, and this essential property also contributes its part. The free periods of chiral twist may or may not be included among the prominent free radiative periods of the molecule. The dispersion would require more than one term completely to represent it.

In a recent important summary (*Comptes Rendus*, July, 1922, p. 174) L. Langchambon reports that, for various organic substances when strictly purified, while the

rotation per molecule has different values in the solid, liquid and gaseous states, yet the ratios of these rotations for three widely separated homogeneous radiations remain the same. Such a result could arise from the condensation, which reduces the effective electric force P of the formula in the ratio $3/(\mu^2+2)$ where μ is the refractive index, and the molecular rotation in the same ratio. If this does not wholly account for the difference we seem to be driven to the conclusion that for these substances, the formula, whatever it be, which represents the rotatory dispersion, contains only one term, as Lowry found in other cases; and that, moreover, in these latter cases that term is preponderantly of molecular type, and not due to any ionic crystalline structure.

3. Prof. A. W. Porter, F.R.S., and Mr. J. J. Hedges.—*The Law of Distribution of Particles in a Colloid Suspension.*

4. Prof. R. Whiddington.—*The Ultramicrometer in Minute Physical Measurements.*

5. Joint Discussion (Cosmical Physics Sub-section) with Sections F and M on *Weather Cycles in Relation to Agriculture and to Industrial Fluctuations.* Opener: Sir W. Beveridge, K.C.B.

6. Prof. Sir William Bragg, F.R.S.—Lecture on *The Significance of Crystal Analysis.*

Friday, September 8.

7. Presidential Address by Prof. G. H. Hardy, F.R.S., on *The Theory of Numbers.* (See p. 16.)

8. Prof. J. C. McLennan, F.R.S.—*X-Rays from Light Atoms.*

9. M. le Duc de Broglie.—*X-Rays and Beta Rays.*

There is a close connection between electrons and light, and this relation reveals itself still more plainly in the consideration of corpuscles of high velocity and vibrations of great frequency.

The equation which appears to govern the interactions between the two phenomena is the quantum equation in the form due to Planck and Einstein, that is to say, we have as between the energy of the corpuscles and the frequency of the vibrations a relation of the form:

$$W = h\nu.$$

I shall only say a word concerning the excitation of X-rays by the impact of cathode rays on the anticathode of a tube; to-day it is well known that the continuous background of the spectrum emitted by an anticathode bombarded by electrons of definite energy W begins on the short wave-length side at a maximum frequency ν, which is precisely equal to

$$\frac{W}{h}.$$

This is a result which Webster has made prominent, and it is completed by the following proposition when the point in question is the excitation of characteristic rays; all the fluorescent rays of a certain series appear simultaneously in a tube when the energy of the cathode rays reaches a value which exceeds the quantum $h\nu$ of the discontinuity of absorption relative to this series. The beautiful experiments of Whiddington formerly gave a first approximation to this law by showing that the emission of Barkla's secondary rays was closely connected with critical values of the velocity of the cathode rays.

The converse phenomenon is now beginning to be well known; here it is a question of the rapidly moving electrons liberated by matter when illuminated by a beam of X-rays. This phenomenon obeys the general law of the photo-electric effects, namely, that the individual energy of the corpuscles expelled only depends upon the frequency and not upon the intensity of the exciting radiation; it is even more remarkably bound up with the levels of energy which the theory of Bohr has pictured in the structure of atoms.

The electrons in number N, which surround the nucleus of an atom of atomic number N, are distributed in a certain number of regions, each characterised by the work which it is necessary to expend in order to remove an electron from the region under consideration and bring it to the exterior of the atom; if the levels of these regions are designated by the letters K, L, M, . . . we can attribute to them energies of extraction having the values W_K, W_L, W_M. . . .

What appears to happen is that if light of frequency ν strikes one of these electrons, situated for example in the region K, it communicates energy equal to $h\nu$ in order to extract the electron from the atom; it is clear that the corpuscle, once removed from the atomic edifice, will possess a resultant energy equal to

$$h\nu - W_K.$$

This, if we wish, we may regard as a form of the equation formerly proposed by Einstein in the case of photo-electric phenomena; Sir Ernest Rutherford, some time ago, used considerations of this nature for the purpose of specifying a possible connection between the natural β and γ rays of radioactive substances.

Certain experimental results obtained by Barkla have led him to believe that all the β rays excited by a beam of mono-chromatic X-rays have the same energy, but other conclusions due to Lewis Simons would point, on the contrary, to the rôle of the levels of energy.

The method of the magnetic spectra of β-ray velocities enables us to analyse the complex bundle of corpuscles emitted by a certain radiator under X-ray illumination; experience shows that one finds there precisely all those groups of electrons which can be anticipated as a consequence of the preceding considerations; each line of the spectrum of the incident X-rays re-echoes on each level of the illuminated atom in such a way that we obtain at once an analysis both of the spectral lines of the illuminating beam and of the Bohr levels of the illuminated atom.

This analysis is made without the intervention of a crystal, that is to say, without encountering the limitation which results from its use in the case of diffraction spectra. It furnishes by means of the measurement of the curvature of a ray and of a magnetic field a method of checking the crystal spacings which have been used in the beautiful work of Sir William Bragg and his son as the basis for the evaluation of the wave-lengths of X-rays.

Mr. Ellis has been able to show that the relation found for X-rays extends also to γ-rays, and makes available for the study of these rays a new device already full of rich promise.

10. Prof. R. Whiddington.—*X-Ray Electrons.*

11. Sir Napier Shaw, F.R.S.—*Convection in the Atmosphere.*

12. Prof. J. Proudman.—Lecture on *Tides, with special reference to the North Sea.*

Monday, September 11.

13. Joint Discussion with Section I on *Physical Instruments for Biological Purposes.* Opener: Prof. A. V. Hill, F.R.S.

14. Prof. H. H. Turner, F.R.S.—*Report of Seismology Committee.* (See p. 253.)

15. Prof. H. H. Turner, F.R.S. described the proposed new 54-ft Interferometer for Mount Wilson Observatory.

16. Exhibition of Physical Apparatus for Biological Purposes.

(*a*) By Major W. S. Tucker. i. *Apparatus for Testing Hearing.*

This apparatus consists of two essential parts: (1) the source of sound, called the transmitter, (2) the receiver.

(1) The source of sound consists of a telephone diaphragm set in vibration by an oscillatory current, generated in a tuned circuit by means of a thermionic valve. The circuit is so designed as to give an exceedingly pure sound, its most efficient note being 500 vibrations per second. The note can be varied in pitch by alteration of the capacity in the circuit.

(2) The receiver consists of a high-resistance Wheatstone bridge circuit, into which the oscillating current is supplied by a silent mercury key. The telephone used for imparting the signal to the ear is placed across the bridge, and the strength of the signal is varied by throwing the bridge out of balance by a measured amount.

A device for measuring ' paracusis ' is included in the receiver. A disturbing sound of constant intensity, capable of reproduction in successive tests, is superimposed on the telephone diaphragm, and the strength of the signal is adjusted as before, until the latter is just audible.

Suitable acuity scales are given by the bridge readings.

ii. *Standard Source of Sound.*

This instrument was designed by Captain Paris to give a sound whose intensity can be measured in C.G.S. units. It consists of a Helmholtz resonator, the base of which is formed by a telephone diaphragm. The diaphragm is excited by an acoustic oscillator, and is of such pitch as to set the resonator in resonant vibration. The sound emerging from the open orifice of the resonator produces a measurable effect on a hot-wire microphone mounted in the neck. Within limits, ohmic change in the hot wire is proportional to the intensity of the sound. A knowledge of the constants of the hot wire, derived from separate experiments, enables one to measure the intensity of the sound.

iii. *Amplifier for Magnifying Sounds such as Heart Beats.*

This is a 4-valve resistance amplifier, in which only one transformer is employed, and is designed to reproduce the lower-pitch microphone currents with as little distortion as possible. It is applicable to the examination of heart sounds.

(*b*) By Mr. F. E. Smith, F.R.S.—*Apparatus for Testing Audition.*

The current in a triode valve circuit is caused to oscillate at an audible frequency, the value of the alternating current being directly measured by an ammeter in the main oscillatory circuit. The frequency of the oscillation is varied by varying the inductance, or capacity, or both, and the intensity of the current is controlled by varying the filament current. Included in the plate oscillatory circuit is the primary coil of an air transformer, the secondary of which is connected with a telephone, thermophone, or other receiver. By varying the intensity of the oscillatory current, or by varying the mutual inductance between the coils of the transformer, or by varying both, the note from the receiver can be rendered inaudible. If the mutual inductance is known, apparatus of this kind can be used as a standard of reference for measurements of acuity. If the amplitude of vibration of, or the total energy emitted from, the receiving mechanism is known, the apparatus can be used for absolute measurement of acuity. To determine the amplitude of vibration a thermophone method can be used, or a piezo-electric crystal can be employed.

(*c*) By Dr. G. Wilkinson.—*Working Model illustrating the presumed Resonating Mechanism of the Cochlea.*

The model consists of a brass box in two chambers (scala vestibule with ductus cochlearis and scala tympani). A window closed by a rubber membrane opens into each. To one of the membranes is attached a small wooden plunger, the ' stapes.' The chambers are divided by a ' basilar membrane ' formed of strands of fine phosphor-bronze flat wire stretched transversely, and plastered over with fine paper saturated with formalised gelatine. The tension on the threads has been regulated by suspending from them a series of weights, graduated according to the formula

$$n = \frac{1}{2l}\sqrt{\frac{t}{db}}$$

where d = the sum of the distances of each wire from the round and oval windows, and b = the breadth of the wire. This formula is an adaptation of the formula

$$n = \frac{1}{2l}\sqrt{\frac{t}{m}}$$

to strings immersed and oriented as in the cochlea. The whole is completely filled with water. It is set into localised resonant action in the calculated positions by applying tuning forks to the 'stapes.' Its compass is four octaves.

(*d*) By The Cambridge and Paul Scientific Instrument Company. i. *Instrument for Measuring the Percentage of Carbon-dioxide in Alveolar Air.*

To physiologists working on respiration and the respiratory functions of the blood it is important to be able to measure the carbon-dioxide in alveolar air. In diabetes, for example, a lowering of the carbon-dioxide is an indication of danger, and the percentage figure is therefore a valuable guide to prognosis and treatment. The instrument shown, suggested by Professor A. V. Hill, provides a convenient and accurate way of measuring this percentage. The patient breathes out in the ordinary way, and then, by an effort, expels through a katharometer of the Shakespear type the residue of the gas in the lungs which has been in actual contact with the blood system. The percentage of CO_2 in this residue is then read on the indicator scale, which is calibrated 0 – 10 per cent. CO_2. The whole outfit is contained in a portable case.

ii. *Salomonson String Galvanometer.*

This is a simple form of Einthoven galvanometer. It has two copper fibres, 0.01 in. diameter. The magnification is about 40 at a working distance of 80 cms. The galvanometer can be used in conjunction with a larger Einthoven galvanometer (fitted with a silvered glass fibre), the optical work in the small instrument forming the eye-piece of the large one. An electro-cardiogram, a phono-cardiogram, and a pulse-tracing can thus be taken simultaneously on the same plate.

iii. *Hydrogen Ion Apparatus.*

The E.M.F. is measured by a potentiometer, in which the slide wire, contacts, and resistances are totally enclosed, thus being protected from corrosion and other deteriorating effects. The instrument reads directly to 0.2 millivolts. It is connected to a moving coil galvanometer, the movement of the coil being observed by a lamp and scale. The potentiometer circuit is standardised by a Weston normal cell. The electrodes are of the Clark pattern, and comprise two hydrogen electrodes, a connecting vessel, and calomel electrode, suitably mounted with rocker and motor.

For blood work a special set of electrodes is supplied.

For electrometric titration work a direct-reading potentiometer is supplied, the instrument consisting of a moving coil galvanometer of the pivoted type, adjustable rheostat, battery, and electrode reversing switch. The E.M.F. of the electrodes is read directly on the scale of the instrument, which is calibrated in millivolts, the standard ranges being 0 – 600 and 0 – 1,200 millivolts.

iv. *MacGregor-Morris Anemometer.*

This is an electrical anemometer designed for measuring the velocity of slow-moving air currents. The instrument consists of a form of Wheatstone bridge, in which two of the arms are made of fine nickel wire. These wires are heated by the passage of an electric current of constant strength, the wire attaining a steady temperature in a few seconds. The heat is then taken away from one of the wires by the motion of the air passing over it, the second wire being protected from the moving air. The heat carried away from the exposed wire by the air current is proportional to the square root of the velocity of the wind. The amount the bridge is out of balance is shown by a direct-deflection galvanometer, the scale of which is calibrated to read directly in air velocities. The method is a sensitive and accurate one, over a range of velocity from 200 to 2,000 cm. per second.

Tuesday, September 12.

17. Mr. H. W. Richmond, F.R.S.—*The Problem of Expressing any Rational Number as a Sum of Powers of such Numbers.*

18. Mr. M. A. Giblett.—*Some Recent Developments in Synoptic Meteorology.* (With reference to the Meteorological Exhibit in the Guildhall, Hull, arranged by the Air Ministry during the meeting.)

19. Mr. J. Jackson.—*Double Stars.*

20. Mr. E. A. Milne.—*The Escape of Molecules from an Atmosphere, with special reference to the Boundary of a Star.*

21. Mr. J. E. P. Wagstaff.—*The Determination of Dielectric Constants and Susceptibilities by Valve Methods.*

22. Mr. J. Ewles.—*Kathode Luminescence and its Relation to States of Molecular Aggregation.*

The experiments were begun with the object of determining whether the luminescence excited by cathode rays appeared only when the rays possessed a minimum speed, *i.e.*, whether a definite quantum of energy was required to excite the kathode luminescence.

The source of high potential was a Mercedes electrostatic machine, and the voltage applied to the tube controlled by the pressure and measured by a Kelvin and White electrostatic voltmeter.

Many metallic oxides and other substances were investigated. It was found that each substance requires a definite speed of cathode rays to excite fluorescence.

The speed is characteristic of the substance, but depends also on the manner in which it is prepared. Evidence is put forward to show that the method of preparation affects the characteristic speed by determining the state of molecular aggregation.

It is suggested that kathode luminescence is a manifestation of the energy change involved when a substance changes from one state of aggregation to another.

To test this hypothesis substances were taken which are known to undergo such a change at a certain temperature.

It has been found in every case that above the temperature of transformation the kathode luminescence disappears.

A striking confirmation was provided by powdered quartz in another way—a change of refractive index corresponding to the formation of a certain amount of tridymite accompanying the fluorescence.

Assuming that the least energy of the cathode rays which excites fluorescence represents the amount of energy required to break up a molecular aggregate, and assuming equipartition of energy among the constituent molecules of the aggregate, it is possible from a knowledge of the molecular heat of transformation to calculate the number of molecules per aggregate.

It is shown that the theory of kathode luminescence here put forward offers a reasonable explanation not only of the above results but of those of other workers on this and other types of luminescence.

Moreover, remembering that fluorescent spectra are always banded, it is hoped, later, to link up this work with the molecular rotation theory of band spectra of emission and absorption.

23. Joint Discussion (Cosmical Physics Sub-section) with Section E on *Monsoons.* Opener: Dr. G. C. Simpson, C.B.E., F.R.S.

SECTION B.—CHEMISTRY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 408.)

Thursday, September 7.

1. Presidential Address by Principal J. C. Irvine, F.R.S. (See p. 25.)

2. Dr. Helen S. Gilchrist.—*The Preparation and Constitution of Synthetic Fats containing a Carbohydrate Chain.*

The products obtained when a carbohydrate chain is coupled with the unsaturated groups characteristic of natural fats have been studied with the object of establishing the constitution of the synthetic fats thus obtained.

As shown by Lapworth and Pearson, α-methylglucoside and mannitol both combine, on heating in the presence of sodium ethoxide, with the oleyl residues of olive oil, thereby liberating glycerol.

The present research has proved that in the first case a mono-oleate is initially formed, whilst in the second two oleyl groups enter the hexitol chain. This condensation is immediately followed by internal dehydration, the carbohydrate chain, in each case, losing one molecule of water, the fatty residues remaining intact.

Anhydro-methylglucoside mono-oleate and mannitan di-oleates are definite chemical individuals. On methylation they yield monomethyl derivatives, which, however, are unstable even in the high vacuum of the Gaede pump. On being heated with acid alcohol these methylated compounds each give methyl oleate, together with an alkylated sugar derivative. In both cases the anhydro-ring in the molecule persists during hydrolysis, and thus a passage is opened into the series of anhydro-sugar derivatives and alcohols.

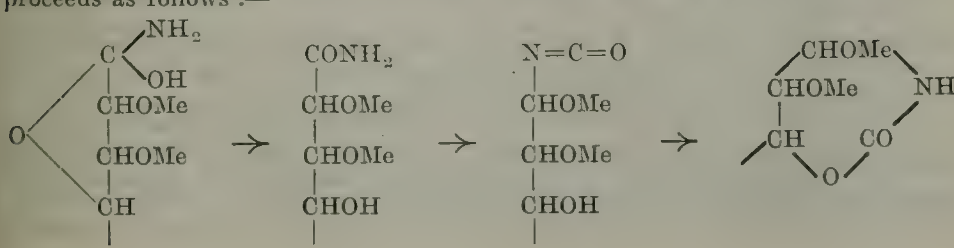
Examination of the above cleavage products confirms the views already held regarding the mechanism of the reactions discussed, and complete structural formulæ are assigned to the original 'methylglucoside' and 'mannitol fats.'

3. Mr. John Pryde.—*A New Type of Nitrogenous Sugar Derivative.*

In extending his studies on the action of sodium hypochlorite on amides of α-hydroxy acids Weerman applied his reaction to the amides of simple hexonic acids, and so devised what has proved to be the best practical method of degrading hexoses to pentoses.

In the present communication the results of applying this degradation method to a fully methylated hexose were given. The investigation was originally undertaken in the hope that a propylene-oxide form of a methylated arabinose would be obtained in place of the normal butylene-oxide type, or alternatively that the intermediate compounds would be isolated and thus elucidate the course of the reaction as applied to the sugar group.

Tetramethylglucose was oxidised to tetramethylgluconic acid, which was isolated as its internal lactone. The lactone, dissolved in absolute alcohol, was treated with dry ammonia and the amide was isolated in a crystalline condition. Evidence is available to show that this compound does not possess the structure of a true acid amide, but exists in the form of an amino-lactone. On subjecting the amide to the action of cold alkaline hypochlorite a crystalline body of the composition of the intermediate isocyanate was obtained, and from its behaviour the constitution of an internal urethane has been assigned to it. The reaction proceeds as follows :—



The formation of this intermediate urethane affords striking evidence of the stabilising effect of methyl groups in the sugar chain. It is also interesting in showing the conversion of a carbohydrate into a derivative in which nitrogen is present in a stable cyclic substituent.

4. Dr. E. L. Hirst.—*The Composition of Esparto Cellulose.*

Esparto grass, after removal of waxes, lignins, &c., in the ordinary course of paper-making, gives a material which is homogeneous and is similar to cotton cellulose in appearance, but differs markedly in that it gives on distillation with 12 per cent. aqueous hydrochloric acid an amount of furfural corresponding to the presence of 18 to 20 per cent. of a pentosan.

Acetylation of this esparto cellulose cannot be effected so readily as in the case of cotton cellulose, but by slight modification of Barnett's method, in which sulphuryl chloride is used as a catalyst, almost quantitative yields of acetates have been obtained without appreciable loss of the pentose residue as shown by furfural estimations. This acetate mixture has been subjected to the action of acid methyl alcohol in sealed tubes at 130° C., when it is found that after prolonged digestion practically the whole of the material dissolves and the solution then contains methylglucoside along with a proportion of a methyl pentoside. The pentose has been identified as xylose, and confirmation of this has been obtained by the isolation from esparto cellulose of a pentosan which on hydrolysis is converted into a reducing sugar identical with ordinary xylose. In the course of quantitative experiments esparto cellulose has thus been converted into methylglucoside and methylxyloside in such a manner that 90 per cent. of the whole material can be accounted for. On the assumption that no other hexose or pentose is present, the analytical results indicate that the overall yield of methylglucoside is 95 per cent. and that of methylxyloside 68.5 per cent. of the theoretical amount. The loss in yield, as indicated by the results of control experiments, is due to the destruction of pentose owing to furfural formation during the digestion in the sealed tubes.

The evidence therefore points to esparto cellulose being, to the extent of 90 per cent. at least, a definite chemical substance composed of glucose residues and xylose residues present together in the proportions of 80 per cent. and 20 per cent. respectively.

5. Prof. Sir William H. Bragg, F.R.S.—*The Crystalline Structure of Organic Compounds.*

6. Joint Discussion with Section K on *Photo-Synthesis.* (See p. 395.)

Friday, September 8.

7. Discussion on *Valency and Polarity in Organic Compounds.*

(*a*) Opener: Prof. R. Robinson, F.R.S.

(*b*) Dr. J. Kenner.—*The Significance of Induced Polarity.*

It is pointed out that Lapworth's derivation of the principle of induced alternate polarities (*Trans. Chem. Soc.*, 1922, **121**, 416) is a further development of the views of Werner and Flürscheim, with the aid of postulates which, subject to apparently reasonable assumptions, conform to the thermodynamic condition for the attainment of stable equilibrium. Fry's electronic theory of valency is based on the fallacy that two atoms in direct combination are necessarily of opposite polarities, and would seem to lead to mutually irreconcilable conclusions when applied to the consideration of certain closely related compounds. The observed results are explicable in terms of the view expressed by Lapworth, and also by Kermack and Robinson (*ibid.*, 427), that polarity is consequent upon constraint of the molecule (resulting, in the cases referred to, from formation of molecular compounds), and on the further assumption that of two alternative reactions that one will predominate in which the free energy gradient is the greater. The important reservation, therefore, seems necessary that before the outcome of a reaction can be predicted by the theory of alternate

polarities the point of constraint must be known. This may be determined by steric considerations, and also possibly by the nature of the reacting compounds. It would appear to follow that (1) the current practice of labelling certain atoms negative or positive is only correct in so far as it indicates the condition they *tend* to assume; (2) the key atom of a molecule is the point at which constraint originates, and hence that one which, in the initial stages of a reaction with a second molecule, is first associated with this. Conceivably, therefore, it may vary from case to case. Attention is drawn to the incomplete nature of explanations which simply refer these to the polarity of a given group. The course of Lapworth's argument explains why considerations of reactions based on Werner and Flürscheim's views have led to correct prognostications. Nevertheless, it would seem in some respects inconsistent with more purely electronic conceptions. In connection with these, it is pointed out that physicists are by no means in agreement as to the details of atomic structure, and that hence it seems premature to associate a discussion of the valency of organic compounds with any one theory. Rather it seems advisable at present to limit discussion to the valency electrons. It is further suggested that a more complete conception of chemical reactions may be gained by considerations of the lines of force associated with electrons. Exception is taken to the mode of derivation of the property of induced polarity employed by Kermack and Robinson, and an alternative is suggested.

It is emphasised that the intermediate production of induced alternate polarities only represents one course by which stable equilibrium is attained. Other factors may contribute to this end, and for this reason a reaction may take a course which would hardly be anticipated from the ordinary way of applying the *plus-minus* notation.

8. Mr. E. D. Williamson.—*The Determination of Compressibilities up to High Pressures and Applications to High-Pressure Chemistry.*

A number of compressibilities have been determined at the Geophysical Laboratory, Washington, in recent years. The method previously used, however, is not sufficiently accurate at the lower end of the pressure range, and so, especially in the case of liquids, some supplementary method is required. A new form of pyknometer has recently been developed and has proved satisfactory in filling the gap. A feature of the instrument is that continuous readings by means of a movable electrical contact may be taken without removal from the pressure chamber.

In the study of the chemical effects of pressure on systems of more than one component it is necessary to know the compressibility of each solution in order to compute the volume changes on which these effects depend. The volume changes can be readily calculated from the slopes of the density-composition curves. Even in the case of a simple system, such as a salt and water, it is necessary to make a number of other measurements in addition to those of compressibility. For instance, good density-composition data must be obtained at atmospheric pressure. Also, some form of equilibrium determinations, such as those of vapour pressure or E.M.F., must be made in order to calculate the initial differences in 'free energy' (used in the same sense as by G. N. Lewis) between the solid salt and salt in solution. For the case of H_2O—NaCl almost all the necessary data for the complete elucidation of the system under pressure have been obtained, and a beginning has been made with some others.

9. Dr. E. F. Armstrong, F.R.S.—*The Hydrogenation of Fats.* (See below, No. 11.)

10. Prof. A. F. Holleman.—*The Rule of the Conservation of Substitution-Type of the Benzene Nucleus.*

1. The rule can only be applied without restrictions to introduction of a second substituent in C_6H_5X.

2. Pure *p-o*-substitutions as well as pure *m*-substitutions must be regarded as limits; all substitutions really observed are more or less mixed.

3. The two types of substitution are not so sharply different as is generally believed.

4. The substitution-type is considered to have remained unchanged as long as there is formed more than 60 per cent. of *p-o*-compounds for the *p-o*-type and more than 40 per cent. of the *m*-compound for the *m*-type.

5. Exceptions seem to be the introduction of ethyl into chlorobenzene by the Friedel-Crafts reaction, where 65 per cent. of *m*-ethylchlorobenzene is obtained, and the mercuration-reactions of Dimroth. These exceptions are only apparent.

6. When there are two substituents, X and Y, already present in the benzene nucleus the validity of the rule can be judged only if the ratios in which the isomerides are formed by the introduction of a second group into C_6H_5X and into C_6H_5Y are known.

7. In the case of the presence of more than two substituents the application of the rule becomes more difficult.

Monday, September 11.

11. Discussion on *The Hydrogenation of Fats.*

Mr. E. R. Bolton.—*Technical Aspects of Hydrogenation.*

The unfortunate state of patent law in connection with chemical processes, as exemplified by hydrogenation, was briefly touched upon.

Factory conditions were then dealt with, commencing with the problem of hydrogen and its preparation (i) electrolytically, (ii) by coke methods, (iii) as a by-product, and (iv) by hydrogen carriers. The relative merits of these methods of production were discussed, passing to the necessary condition of the oil for the most economic and efficient treatment, and dealing with catalyst poisons and substances affecting the life of the catalyst. The catalyst itself, it was pointed out, will always be the item of greatest importance, and the processes whereby so much catalyst per ton is regularly lost will rapidly die, to be replaced by those in which the catalyst is considered as a capital charge—in fact, a part of the plant. Emphasis was laid upon the necessity of an efficient and skilled scientific staff for the control and preparation of the catalyst and the examination of the raw materials used. Physical factors were then discussed briefly, including the engineering side and comprising problems of contact, solubility of hydrogen in oil, and, finally, types of plant, in which connection it was pointed out that it is only the continuous plant which is likely to find favour in the future. Attention was drawn to the connection of all these issues with the cost of hydrogenation.

The different types of product required in the various industries, such as the edible oil, soap, and candle industries, were dealt with, and, finally, suggestions were put forward for further research and an indication of probable developments made.

12. Dr. R. Whytlaw Gray.—*Gaseous Dispersoids.*

13. Prof. J. W. McBain.—*The Study of Soap Solutions.*

In view of the large number of current theories which are irreconcilable with the conclusions to which the study of soap solutions has led it is necessary very carefully to prove the evidence for the existence of the ionic micelle and the theories arising therefrom.

1. The following methods agree in showing that hydroxyl ion is only a minor constituent of soap solutions, being only about $0.001N$:—

(*a*) Electromotive force with hydrogen electrode.
(*b*) Catalysis.
(*c*) Conductivity.
(*d*) Ultra-filtration with direct analysis of the filtrate.

2. The osmotic activity is about half that of a salt. Trustworthy measurements have been obtained by :—

(*a*) Freezing-point.
(*b*) Dew-point.
(*c*) Minimum pressure required for ultra-filtration.
(*d*) Vapour pressure.

3. The conductivity is that of a salt, illustrated by hundreds of concordant measurements with many kinds of soap.

4. Half the conductivity must be ascribed to a constituent of very high equivalent conductivity but of negligible osmotic pressure, the ionic micelle. Its conductivity is several-fold that of all the fatty ions contained in it.

5. By ultra-filtration the ionic micelle is found to be colloidal, and, in addition, the undissociated neutral colloid, consisting of still larger particles, may be separated from the ionic micelle. Sodium and potassium electrodes confirm the concentrations of sodium and potassium ions assumed. Migration determinations are also in agreement. Hydrolysis is impossible in the case of cetyl sulphonic acid, whose behaviour is closely similar.

6. An important result is the theory of gel structure. The only differences between a transparent jelly and a sol are mechanical—elasticity and rigidity. The colloidal particles in both are identical in nature and amount, but in the gel they are arranged in ultra-microscopic filaments or aggregates. The equilibria and the resistance to the passage of the electric current are unaltered on gelatinisation.

7. 'Electrical endosmosis' and 'cataphoresis' in a transparent soap jelly are quantitatively identical with electrolytic migration in the corresponding soap sol.

8. The theoretical conclusions are of general applicability to very large groups of organic and inorganic solutions in aqueous and non-aqueous solution.

Tuesday, September 12.

14. Discussion on *The Nitrogen Industry.* (See p. 415.)

15. Dr. J. S. Owens.—*Atmospheric Dust.*

16. *Report of the Fuel Economy Committee.* (See p. 277.)

SECTION C.—GEOLOGY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 408.)

Thursday, September 7.

1. Discussion on *The Geological History of the North Sea Basin.*
(*a*) Opener: Prof. P. F. Kendall.

The North Sea is situated upon an area of very ancient and persistent depression, dating probably from Permian times. Coal-measure rocks dip into the Basin in Belgium and Holland, north of the anticline of Brabant; probably in Lincolnshire; and certainly in Durham and Northumberland: they re-emerge at Ibbenbüren.

The Permian rocks of Yorkshire and Lincolnshire increase steadily in thickness from west to east. The Triassic rocks show no significant change. Members of the Jurassic and Cretaceous series attain their maximum development in Britain in the area bordering the North Sea, and the Lower Cretaceous beds exhibit a deep-water phase absent elsewhere in Britain. The chalk thickens north and east of London partly by preservation of higher zones, and partly by general expansion. In Yorkshire zone for zone it reaches its maximum development, but in a deposit of this type this may not indicate contemporaneous movement.

The renewal of the Armorican movement in the South of England, Northern France and Belgium appears to have begun after the deposition of the London Clay and Woolwich Beds, and after some interruptions to have resumed its activity at or about the beginning of Pliocene (Diestian) time. This may be regarded as the first appearance of a North Sea as we know it.

Harmer has suggested a coast-line connecting the Lenham and other outliers spanning the Straits of Dover, joining up with the main Diestian outcrops of Belgium. This view is accepted by the Belgian geologists and accords generally with my own judgment, though not without some reservations. This may be

the stage represented by Barrow's High Level (Pliocene) Gravel of the London Basin, which seems to belong to a period prior, as he believes, to the great denudation.

This, then, would be the western embayment of the North Sea.

A movement *en bascule* seems to have ensued, resulting in a general but intermittent uplift on the English side and along the axis of Artois, with a complementary slow depression of Northern Belgium and Holland. Great denudation of the British Diestian, and probably some Miocene, ensued, and the Coralline Crag, probably an offshore shoal in 20 or 30 fathoms of maximum depth, was accumulated upon an eroded surface of Lower Eocene; but its gravelly base obtained the coarse scourings of London Clay, (?) Miocene, and fossiliferous concretions of a sandstone near to the Diestian in age.

On the Belgian side depression seems to have been almost continuous.

The Coralline Crag was uplifted and subjected to severe sub-aërial and marine erosion, and the Red Crag overlapped it on the English and Belgian sides.

With the earliest Red Crag, that of Walton-on-the-Naze, the record becomes practically continuous. Steady uplift was taking place in the south, throwing the coast-line farther and farther north. Harmer recognises three stages, but many more could be distinguished by the progressive changes in the fauna—the elimination of Southern types and the incoming of Northern.

The Norwich Crag comes in north of a ridge of Coralline Crag at Aldeburgh and appears to represent a very slightly more modern phase than the Red Crag flanking the ridge on the south.

Chillesford Sand and Clay surmount the Red and Norwich Crag from Essex upward.

Harmer regards them as the deposits of a winding estuary of the Rhine.

A renewal of the 'Crag' type of deposition, the Weybourn Crag, with *Tellina balthica*, was succeeded by a definite estuarine series, the Cromer Forest Bed. It begins with the Freshwater Bed, in which a flora comparable to that of Norfolk to-day is found. This is followed by a marine deposit with Arctic shells, and above that a second freshwater bed with an Arctic flora.

This series may rest directly on the Chalk, an effect of the continued upward tendency on the English side of the sea.

In Holland the whole Pliocene series is probably present, and the great thickness of comparatively shallow-water deposits prevailing down to 1,100 feet at Utrecht bespeaks a continual downward tendency of the Low Countries.

The evidences of Pliocene conditions further north are extremely scanty, in fact only near Hartlepool, where Trechmann has found a pre-Glacial plant bed, is there any relic of Pliocene deposits in their native position.

At Sheringham Mr. Stather has found a block, doubtless from the Drift, of Red Crag of about Newbournian date of a type unknown elsewhere.

In Holderness the Drift has yielded *remanié* fish remains evidently derived from the Red Crag, and in Aberdeenshire in the Slains Gravels Jamieson found a large suite of rather fragmentary shells of mainly early Red Crag facies, mingled with some undoubted Pleistocene forms.

The Cromer Forest Bed stage is the latest pre-Glacial stage recognised in East Anglia, but in Yorkshire a well-defined cliff-line bounding a broad plain of marine erosion is traceable, which appears to be, in part at least, of later date.

The cliff begins at Sewerby between Bridlington and Flamboro Head, and has been traced by Mr. Crofts and myself round to Hessle, near Hull. The corresponding beach has been seen at each end. Borings have enabled the old sea-floor in front to be charted and contoured.

The next phase was a retreat of the sea and formation of sand dunes along the foot of the cliffs. The geological date is indicated by the occurrence of *Elephas antiquus*, *Rhinoceros leptorhinus* and *Hippopotamus* in the deposits.

This fauna accompanies implements of Chellean type in the South of England.

The next episode was the arrival of a great ice-sheet having its radiant point in the neighbourhood of the Gulf of Bothnia. This appears to have displaced the water from the whole of the North Sea as far south as the coast of Essex, if no further.

Several retreats and readvances took place, and the final retreat can be traced with great detail and precision by the drainage phenomena developed along

the margin of the ice up to its last contact with the British shores on the Ord of Caithness. Oscillations of level accompanied the retreat and raised beaches were left, especially north of the Tweed, but on the completion of the withdrawal the land stood about 80 feet higher relatively to the sea than at present. The southern portion of the North Sea became a marshy plain, over which great rivers such as the Rhine, Thames and Weser took a meandering course. Peat bogs occupied much of the area, and forests clothed the margins.

A depression to the present level then ensued and the great shallow bay of the North Sea south of the Dogger Bank was formed. Only in a few places does its depth exceed 25 fathoms. The sea ran up the estuaries, and thus the Humber itself and its branch the Hull came into being.

A true scale of the breadth and depth of the North Sea in this latitude can be obtained by taking a piece of No. 40 sewing cotton 13 ft. 8 in. in length. Knots tied in the cotton would represent Heligoland and any of the ' pits.'

(*b*) Mr. J. O. Borley.—*The Floor Deposits of the North Sea.*

2. Mr. Thomas Sheppard.—Lecture on *The Geology of the Hull District.*

3. Mr. C. Thompson.—*The Erosion of the Holderness Coast.*

An attempt has been made to indicate the strip of land lost by erosion from the coast of Holderness during the past seventy years. A reliable average loss per year has also been sought for.

The plan adopted was to take the Ordnance Survey map (6-inch scale) of the coast, to measure various well-defined lines on that map, and to measure the remnants of those lines in the field.

These measurements have been plotted on the 1852 map and the present coast-line drawn approximately, so that the actual loss may be readily visualised.

The averages for the seventy years are somewhat less than the usually accepted rate.

Friday, September 8.

4. Professor A. P. Coleman.—*Pleistocene and Recent Ice Conditions in North-Eastern Labrador.*

The Labrador ice sheet of the Pleistocene left uncovered some thousands of square miles of North-eastern Labrador, in what has been called the Torngat Mountains, the lofty edge of the Archæan Shield. There were, however, large valley glaciers carving up the edge of the Shield with deep valleys and fiords, giving the wildest and most mountainous region of Eastern North America.

At present there are about fourteen small cirque glaciers on or near the coast. On the interior plateau there are numerous small stagnant ice sheets which are doing no geological work.

The present climate, though severe, with less than two months of summer, is semi-arid, providing little moisture for precipitation as snow. During the Ice Age the snowfall must have been much heavier, implying much moister conditions.

5. Mr. J. W. Stather.—*A New Section in the Oolites and Glacial Deposits at South Cave.*

The old railway cutting in the Millepore Oolite, west of South Cave Station, has recently been quarried back and exhibits some unexpected features. The most important of these is the occurrence of an irregular band of flinty and chalky rubbly drift, above which is a mass of displaced Millepore Limestone, partly shattered, but in places maintaining its original bedding, almost giving the impression of being *in situ*.

The presence of the Millepore Limestone above the chalky rubble can only be explained by supposing the limestone to have been carried over the newer deposits by some transporting agency, presumably glacial, as there is no place in the immediate neighbourhood from which the Millepore Limestone could have slipped by gravity into its present position.

6. Presidential Address by Prof. P. F. Kendall, on *The Physiography of the Coal Swamps.* (See p. 49.)

7. Prof. A. Gilligan.—*Sandstone Dykes or Rock-Riders in the Cumberland Coalfield.*

Those more particularly described were met with in the workings of the Bannock Band and Main Band seams at Ladysmith Pit, Whitehaven. The dykes certainly pass through the Bannock Band and Main Band seams and the intervening measures, which are about 54 feet thick; but their full vertical extent has not been determined. Their horizontal extent is variable; the longest has been traced for more than a mile. They all run practically parallel to one another in a direction N.N.W. and S.S.E., which is the direction of the main system of faults affecting the associated measures. The average width of the dykes is from two to four inches and the coal in sharp contact with them is unaffected.

The dykes do not pass up into the overlying Whitehaven sandstone.

The author argues for the pene-contemporaneous formation of these dykes and the associated measures.

Monday, September 11.

8. Discussion on *Wegener's Hypothesis of Continental Drift.* (*a*) Opener: Dr. J. W. Evans, F.R.S.

The occurrence of allied forms of life on continents separated by great oceans has given rise to speculations as to former connections between them. There is in some cases also considerable similarity in the geological features on the opposite sides of the oceans, especially in the case of the Palæozoic rocks of the two sides of the Atlantic and of the Permo-Carboniferous formations of India, Australia, South Africa, the Falklands and South America. It was, however, primarily to solve the distribution of life forms that Dr. Wegener formulated his hypothesis of the dispersion of the continental masses. He supposes these to have been aggregated together as late as the Tertiary period, and North America to have been in close proximity to Europe even in the Pleistocene. The continents are, he thinks, slowly drifting from the poles, and from east to west. The former movement is believed by him to be proved by observations at European observatories and at that at Washington, showing a decrease in latitude, but recent observations on the Pacific coast indicate that latitude is there increasing. He is under the impression that America is going west faster than Europe, and that the longitude of Cambridge, Mass., is increasing while Greenland is moving in the same direction at a still more rapid rate. The continents are composed largely of rocks rich in silica and alumina, 'Sial,' while those below the sea are less siliceous and heavier and have been referred to as 'Sima.' This extends under the Sial of the continents at a depth which Wegener estimates at 57 miles, which is probably far too great. He believes that the Sial masses drift through the Sima like icebergs through the sea. The Sima is, however, a crystalline solid down to a depth of 15 miles, and is as strong as, if not stronger than, the Sial.

By overthrusts and crumpling some areas are brought nearer together, and by fissuring, igneous intrusions and normal faulting others are moving apart. There is some evidence of a slow drift away from Africa and towards the Pacific, but there is nothing to show that this has been as rapid as is assumed by Dr. Wegener. The Atlantic may have come into existence since Carboniferous times, but this would not mean a separation equal to its whole width, as part of the submergence would be caused by faulting down towards a region of tension.

The Astronomer-Royal of Scotland has shown that determinations of longitude by telegraphy are subject to serious errors. Light is thrown on this by the observations of Hecker and others with horizontal pendulums. In addition to the variations of direction of gravity due to the sun and moon, the earth's crust is subject to appreciable diurnal tilting as the result of solar radiation. Similar seasonal effects must also occur. Variations of the barometer in adjoining areas and of the underground water-level, marine tides and irregularities in refraction, are also important. Most of these effects are considerably less at some depth below the surface, and it is suggested that the

instruments employed should be placed in excavations and protected from disturbing influences. They should be located in plains with uniform geological structure and surface character.

(*b*) Prof. H. H. Turner, F.R.S.—*The Astronomical Evidence Bearing upon the Hypothesis.*

The only piece of astronomical evidence supporting Wegener's hypothesis, and worthy of serious consideration, is the apparent drift of Greenland. The observations in 1870 and 1907 show a change of 1,200 metres, and observations in 1823 lend some support, but it cannot be said that the drift is established beyond doubt, though a good case is made for repeating the observations to-day; indeed, the matter is so important that this is a duty. All the other evidence is practically against such changes in modern times.

(*c*) Mr. W. B. Wright.

A critical comparison of the geological formations on the two sides of the North Atlantic shows on the whole a very remarkable correspondence, both stratigraphical and palæontological, from the Pre-Cambrian up to the Cretaceous, and in particular brings to light certain facts even more strikingly indicative of a former *rapprochement* between the two continents than any pointed out by Wegener.

The recurrence in America, on opposite sides of the old Appalachia, of the two facies of the European Cambrian and early Ordovician, which are here separated by the Caledonian chain, is perhaps the most striking, the lithological and faunal distinctions and the sequences of transgression and recession, different on either side of the chain, being reproduced with remarkable precision. Again, the continental and marine facies of the Devonian are separated in both countries by boundaries which become conterminous on the Wegener reconstruction.

The equivalent line in the Triassic lies further south both in Europe and America, but, as it passes into the areas of generally defective correspondence in Spain and Central America, is less valuable as a criterion. It should, on the other hand, be noted that in America there are in the middle of the Carboniferous and Cretaceous formations marked unconformities which have not been recorded in Europe. The investigation of these discrepancies, perhaps more apparent than real, might well form a test case for the theory.

9. Dr. Herbert L. Hawkins.—*The Relation of the River Thames to the London Basin.*

(i) The London Basin is an asymmetrical syncline pitching eastwards. The southern rim dips sharply, the northern gently, the axis lying nearer the south. Drainage of such a district would consist normally of a main stream along the axis with tributaries on both sides. This condition is realised in the west by the Kennet and in the east by the Thames below Chertsey. In the intervening portion (Theale to Chertsey) the main stream is north of the 'ideal' position, and its southern tributaries cross the axis and flow against the dip At Wargrave the Thames returns into the Chilterns in apparent defiance of all rules and reason. There is evidence that the southern tributaries have postponed junction with the main stream fairly recently, and that the Thames itself has shifted its course southwards.

The original drainage of the basin is believed to have passed along the Kennet to Theale, thence to Pangbourne, along the present Thames to Windsor, thence by Rickmansworth and Hertford, and probably down the Lea. (Possibly it reached the sea past Maldon or Colchester.) These broad meanders were reversible with similar amplitude. This line was probably the synclinal axis in Miocene and Pliocene times, its southward displacement being due to the increased plunge of the 'North Downs' dip. Many anomalies in the drainage and river deposits are explicable on this hypothesis, the rivers being incompletely adapted to the tectonic change.

(ii) The Upper Thames is a tributary of the London Basin drainage. New sections in Goring Gap show that torrent action has deepened the gorge by about fifty feet. Excavations on the hills above Whitchurch afford presumptive evidence of the marginal effects of true glaciation. The nature and arrangement

of the drift, the numerous lateral channels, and the character of the gorge itself, are such that their occurrence further north would be ascribed generally to glacial action.

It is postulated that a Welsh-Midland ice-sheet reached the chalk-scarp, but failed to override it save at the lowest parts. One such part was in the angle between the Chilterns and the Berkshire Downs; the final excavation of Goring Gap was achieved by the outflow from a retreating ice-tongue that had penetrated into the London Basin. The relatively early date of the glaciation and the softness of the bed-rocks account for obscurity of details.

10. Joint Discussion with Sections E and H on *The Relation of Early Man to the Phases of the Ice Age in Britain.* Opener: Mr. H. J. E. Peake.

Tuesday, September 12.

11. Mr. W. S. Bisat.—*Goniatite Zones in the Middle Carboniferous.*

Renewed attention paid by members of the Yorkshire Naturalists' Union to this question has shown that all species of the principal goniatite genera are very limited in vertical range, and this fact has rendered it possible to greatly expand the sequence propounded by Wheelton Hind.

The zones suggest strongly that the Millstone Grit series of North Derbyshire represents only the upper part of the North Yorkshire and Lancashire series, and that the Yoredale Shales of Derbyshire form the equivalent of the Sabden Shales of Lancashire. This brings the Third Grit of Lancashire and Yorkshire approximately on the same horizon as the Kinderscout of Derbyshire.

12. Mr. R. G. Hudson.—*A Type Section of the Yoredalian.*

Skell Gill and Mill Gill, Wensleydale, Yorkshire, show a complete and typical exposure of the Yoredale rocks of the Lower Carboniferous.

The palæontological zones established elsewhere for the Lower Carboniferous are confirmed in this section and further subdivision is possible. The conditions of deposition are emphasised by the occurrence of shallow-water faunas and algal horizons. Local unconformities occur, notably between Dy and D_2, where the Hardraw shales rest on the eroded and irregular surface of the Gayle Limestone.

The fauna reaches its maximum development in the Main Limestone, where the corals and brachiopods show great specific variation, and are followed by a stunted and impoverished fauna. Many horizons are characterised by faunas that are, as elsewhere, constant throughout the district, notably *Girvanella* and *Orionastrea phillipsi*, characteristic of the base of D_2 and Dy_2 respectively.

13. Mr. W. S. Bisat.—*A New Section in the Oolites at North Ferriby.*

A well sinking and boring and quarry operations about nine miles west of Hull, on the edge of the Wolds, gave good sections from the Middle Chalk to the Lias. The junction of the Cretaceous and Oolite series showed Red Chalk and marl resting on pebbly Carstone.

The ammonite fauna of the underlying clays was submitted to Dr. Spath for determination. The clays have always been referred to the Kimmeridge, but the ammonite evidence showed that they are undoubtedly Corallian, and belong to the Ampthill phase.

This indicates a greater gap between the topmost Oolites and lowest Cretaceous than had been supposed to exist in Yorkshire.

SECTION D.—ZOOLOGY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 408.)

Thursday, September 7.

1. Dr. Johs. Schmidt.—*Remarks on the Life-history of the Conger, being a Preliminary Communication from the 'Dana' Expeditions in the North Atlantic.*

2. Mr. J. Gray.—*The Mechanism of Ciliary Movement.*

The cilia and the gills of *Mytilus* are independent of any nervous control. Their rate of beat depends upon the following factors : (i) Temperature; (ii) degree of alkalinity of the cell interior ; (iii) presence of oxygen ; (iv) concentration of potassium ions ; (v) presence of calcium. The amplitude of the beat depends upon the amount of water in the cell. These and other facts indicate that the actual contraction is due to the uptake of water by the fibrils when an acid is liberated at their surface. Since the amount of oxygen consumed by the cells is controlled by the same factors as those which control the rate of beat, and since the cells are impermeable to anions, it seems likely that the oxygen is used to remove the products formed by the acid when it diffuses away from the fibrils during the period of relaxation.

3. Dr. G. P. Bidder.—*The Relation of the Form of a Sponge to its Currents.*

Fixed organisms, of animal physiology, depend for nutrition and respiration on the efficiency of the currents which bring them food and oxygen. The outgoing current forms an angle with the incoming streams, which may be called the angle of supply ; between them, unless this be 180 degrees, is a revolving eddy whose diameter may be called the diameter of supply. Through this eddy, in still water, the incoming streams are polluted from the outgoing stream : the proportion of food-bearing and unpolluted water which reaches the organism increases when either the angle or diameter of supply increases. In sponges (excepting Hexactinellida, which live in immutable bottom-currents) all progressive modification of internal canal-system, or external form, demonstrably increases diameter of supply or angle of supply. Determination of velocity in different regions of a canal system shows them proportional to the needs of a perfect hydraulic engine.

4. Dr. Alexander Bowman.—*The Biological Interchange between the Atlantic and the North Sea.*

During the course of the investigations being carried out by the Fishery Board for Scotland in their Research vessel in the autumn of 1921, special attention was directed to the abnormal hydrographic conditions prevailing in the northern part of the North Sea adjacent to the coasts of Scotland. As also in 1920, an unusual incursion of Atlantic water into the North Sea had occurred. That such occurrences should have profound biological significance will be evident. Apart from the changed physical conditions, such as an increased temperature and higher salinity, the invading water masses bring with them a whole series of organisms not usually found in the North Sea, of which the most striking are perhaps the *Salpæ* (and more particularly *Salpa fusiformis*). Unusual conditions such as these offer a fruitful ground for research into the probable causation of the distribution of various pelagic organisms, more particularly those belonging to the passively carried plankton. It would also appear that there is some evidence in support of the view that the failure of the great herring-fishery in these two years is not unconnected with these phenomena.

5. *Reports of Committees.*

(*a*) Zoological Station at Naples. (See p. 315.)
(*b*) Marine Laboratory, Plymouth.
(*c*) Zoology Organisation.
(*d*) Zoological Bibliography and Publication. (See p. 316.)
(*e*) Inheritance of Colour in Lepidoptera. (See p. 318.)
(*f*) Inheritance in Silkworms.
(*g*) Parthenogenesis. (See p. 317.)
(*h*) Gilbert White Memorial.

6. Discussion on *The Fauna of the Sea-Bottom.*

(*a*) Opener: Dr. C. G. Joh. Petersen.

I confine myself to discussing methods of investigation. From 1883 I carried out investigations on the Bottom-Fauna of Danish waters by means of the dredge, and charted the occurrence of each single specimen. No good general survey of the animals existed at that time; different groups of animals were treated by different specialists who published in different papers. Later I introduced the bottom sampler for the purpose of investigating the problem as to why the plaice grows faster in some parts of the Limfjord than in others. The conception of the 'fauna of the level bottom' and that of the 'epifauna' was introduced, as also that of 'animal communities' based upon characteristic animals. The theory of probability must be applied to this line of investigation, the degree of exactitude depending upon the number of stations worked. Seasonal and annual variations. Bearing upon the fisheries (plaice). Knowledge of Metabolism of the Sea, based upon quantitative investigations of the bottom fauna and flora. Dredge rather capricious, mixing communities together and confusing level-bottom fauna and epifauna.

(*b*) Mr. F. M. Davis.

During the last year work has been carried out in the North Sea with the Petersen Bottom Sampler with a view to finding out the distribution of the non-predatory benthos forming the food of fish. The work has been mostly concentrated on the central area of the Dogger, where 391 stations have been worked, while 145 others have been worked in other areas. The following are the commonest animals found: *Spisula subtruncata* (maximum 3,280 per square metre), *Mactra stultorum* (maximum 55), *Tellina fabula* (maximum 40), *Natica alderi* (maximum 40), *Nephthys cæca* (maximum 25), *Echinocardium cordatum* (maximum 3). Many other animals—*e.g.* *Venus gallina*, *Psammobia ferrœnsis*, *Syndosmya prismatica*, *Ophelia limacina*, *Goniada maculata*, and other forms—are widely, though not intensively, distributed, not usually occurring more than 5 per square metre. This appears to coincide approximately with Petersen's Danish 'Venus' and 'Deep Venus' communities, but the predominant species are *Spisula subtruncata* (intensively distributed) and *Nephthys cæca* (extensively distributed). It has been found that *Spisula subtruncata* and the more intensively distributed animals lie in patches. In a recent voyage a patch of *Spisula* was intensively sampled, and was found to extend over an area of approximately 38 square miles, with an average intensity of 795 half-grown individuals per square metre (maximum 2,520, minimum 0).

(*c*) Mr. J. O. Borley.

A discussion of the distribution of the sea-bottom fauna in the North Sea on the basis of samples obtained by the dredge.

(*d*) Prof. R. D. Laurie, Miss E. Horsman, and Mr. E. E. Watkin.—*The Fauna of Cardigan Bay, off Aberystwyth.*

Mechanical analyses of samples of the bed of a trawling ground off Aberystwyth give the following as a typical result: coarse sand 4.5 per cent., fine sand 30 per cent., silt 19 per cent., fine silt 20 per cent., clay 11 per cent., soluble matter, etc., 14 per cent. A typical collection of organisms obtained by the Petersen Bottom Sampler in $\frac{1}{10}$ sq. metre is: *Amphiura filiformis* (21), *Turritella communis* (7), *Synapta inhærens* (1), *S. digitata* (1), *Pectinaria auricoma* (2), *Nephthys cæca* (2), *Sthenelais limicola* (2), *Evarne impar* (1), *Phascolosoma procerum* (1), *Montacuta bidentata* (1), *Syndosmya alba* (1), *Corbula gibba* (1), and other forms less frequently, among them *Echinocardium cordatum*. This Amphiura-Turritella 'community' does not correspond to any of Petersen's. Among the immature fish trawled on this ground, plaice and skate do not appear to be serious competitors with one another for food; the former having a marked preference for the brittle-star, *Amphiura filiformis*, whereas the latter avoid it but specially select the Amphipods, *Ampelisca typica*

and *A. lævigata*; *Turritella communis* is avoided by both. A Paludestrina-Cardium-Macoma community in the littoral zone of the Dovey Estuary is also under investigation: plaice caught over this region contained *Cardium edule*. Curves indicate the period of growth and rate of growth of various forms, and fluctuations in the frequency of their occurrence in successive years. In the cockle the fluctuations are particularly striking, and an attempt is made to throw some light upon them.

(*e*) Dr. JOSEPH PEARSON.—*The Growth-rate of Placuna Placenta.*

The account was based on a considerable mass of data from Ceylon waters, and was a summary of twelve years' work. Illustrative curves dealing with linear growth and with weight were exhibited.

7. Dr. JOSEPH PEARSON exhibited diagrams showing a remarkable series of seasonal changes in the salinity of Lake Tamblegam, a large marine area opening by a narrow entrance from the harbour at Trincomalee, Ceylon.

8. INSPECTION OF RESEARCH VESSELS.—The members of the Section inspected the research vessels visiting Hull during the meeting—namely, the Danish Government vessel *Dana*, the Scottish *Explorer*, the *George Bligh* of the English Ministry of Agriculture and Fisheries, the *Salpa* from the Marine Biological Association at Plymouth, and the *Evadne* from Cullercoats.

Friday, September 8.

9. Presidential Address by Dr. E. J. ALLEN, F.R.S., on *The Progression of Life in the Sea.* (See p. 79.)

10. Discussion on *The Sea Fisheries.* (*a*) Opener: Mr. J. A. ROBERTSON, J.P., O.B.E., on *Fluctuations in the Fisheries.*

There have always been apprehensions that the stock of trawl fish was diminishing through over-fishing. Since 1853 a variety of Commissions have investigated the matter, Acts of Parliament have been passed and repealed, but no authoritative finds have been reached. The present paper suggests that rather than there being a progressive diminishing of the stock of fish due to fishing operations, there are upward and downward fluctuations about a mean which are of more importance than are human agencies due to such factors as temperature and food supply. An increase in the intensity of fishing may lower the mean about which the fluctuations occur, but the degree of lowering will not be material. An example is given of what is believed to be a case of a fluctuation due to natural agencies. Hake had not been recorded in any abundance on the Morecambe Bay grounds before 1899, but in that year large hauls were made by a few boats, which secured still greater catches in 1900; but in 1901 very few were caught, and by 1905 they had entirely disappeared and have not reappeared since. In this case there was no question of the gradual depletion of a regularly fished ground. Again, the fishing trade had made plans to deal with considerably increased catches in the North Sea, which it was anticipated would follow the rest period of the War. It was calculated that in 1920, when the trawling fleet had got back to its full strength, catches would be double the pre-War maximum, but the quantity landed was actually less than a third of what was anticipated. The main obstacle to the solution of this difficult problem is the lack of reliable statistics. The present method of collecting them is untrustworthy. At Grimsby, for example, erroneous statistics were known to be given to the Ministry's collectors, so that trade grants might be preserved, and so in conclusion a strong plea is made for co-operation between the Ministry and the industry for the drawing up of a new

scheme making owners responsible legally for supplying correct statistics, for without such it would appear impossible to attain any final conclusion on this problem of fluctuations, which is of crucial importance to the fishing industry.

(*b*) Mr. H. G. Maurice, C.B., supported the present system of collecting statistics. The Ministry employed whole-time collectors in all the principal ports, whose sole business it was to collect fishing statistics, and he did not think any different results would be obtained were the owners to take the matter in hand. The fishing trade was, he added, always urging the Ministry to investigate, and then when they did so and results were given they were told they were wrong. He might allude to proposals relating to plaice which had recently been made by the Ministry as a result of investigations and which were turned down by the trade

(*c*) Prof. J. Stanley Gardiner, F.R.S., suggested that if the Ministry and the industry came together in this matter they would get statistics which were more economical, more accurate, and more such as the industry required.

(*d*) Dr. E. S. Russell admitted that natural fluctuations occured in all great fisheries, but he could not agree with Mr. Robertson that the effect of steam trawling was negligible. He brought forward evidence based upon North Sea data to drive home his point.

(*e*) Prof. James Johnstone admitted that some effect might result from intensive fishing, but believed that his investigations in the Irish Sea pointed to the considerably greater importance of natural periodic fluctuations.

(*f*) Mr. B. Storrow and Mr. A. Robins also contributed to the discussion.

11. Discussion on *The Sea Fisheries, with Special Reference to the Herring.*

(*a*) Opener: Mr. George Hall.

(i) Introductory remarks of a general character.

(ii) Reference to the universe and the many relative questions crying out for research and solution.

(iii) Narrative of the speaker's connection with the herring industry, in the course of which particulars are given of the nature of the problems which have presented themselves, and to which, hitherto, there has been no satisfactory answer.

(iv) Attention directed to the condition of things brought about by our ignorance of the causes of the varying circumstances under which the industry has been operated.

(v) Suggestions as to the direction in which the assistance of the industry might prove effective, with particular reference to a system of co-operation between the industry and science in supplying information respecting the following matters: (*a*) Place of catch, (*b*) time of catch, (*c*) state of tide, (*d*) state of weather, (*e*) conditions of water, (*f*) means of catch, (*g*) method of catch, (*h*) description of catch under the headings of—(i) composition, (ii) predominating characteristics, (iii) exceptional characteristics, (iv) measurements.

(vi) Favourable results anticipated from complete knowledge of the herring and its haunts.

(*b*) Mr. David T. Jones, C.B.E.

The phases through which the herring fishery has passed are (1) the period when the bounty system was in operation; (2) the introduction of the 'Zulu' type of boat, which possessed excellent seagoing qualities and enabled the fishermen to use a much larger fleet of nets; and (3) the revolutionising introduction of steam and motor power. Regarding research, an inquiry was made as far back as 1837, following the failure of the estuarine fishery in the Firth of Forth, which led to a distinction being drawn between the sprat and the young herring. Specimens dating from this investigation were exhibited. A further investigation carried out by the Board in conjunction with the Meteorological Society of Scotland in 1873-75 had led to the conclusion, which has been confirmed by subsequent investigations, that local variation in the temperature of the sea has a very important bearing on the abundance of herring. Recent

investigations upon herring scales suggest the possibility of predictions as to the abundance or otherwise of the fish in particular years, in the absence of any unforeseen disturbing factors.

(c) Prof. Otto Pettersson.—*On the Periodic Character of the Scandinavian Herring-fishery.*

The essence of my paper is to draw attention to the great secular periodicity in the movements and the circulation of oceanic waters and their influence on the migrations of fishes (the herring) which have occurred in our parts of the sea once in every century during the last thousand years.

(d) Mr. B. Storrow.—*Herring Fluctuations.*

There is a relation between tidal phenomena and the productivity of the herring-fishery of the East Coast of Scotland. Tidal phenomena precede herring catches by four years, and the curves show, alternately, periods tending to parallelism and convergency. Data for catches arranged in a nine-year period show that the fourth year after the greatest tide-generating force exerted by the moon (Pettersson) is marked by high catches. There is a period of 18-19 years in the catches of winter herrings off the East Coast of Scotland, and probably in the East Anglian fishery. The poor quality of the herrings of 1920 and 1921 and the large numbers of young spawning fish in the shoals were due, probably, to the same factors which determined the liver yield from, and young fish amongst, Norwegian Skrei. It is probable that further work will enable us to foretell some of the good summer fisheries.

(e) Dr. Wm. Wallace.—*On the Spawning and Early Stages of the Herring in the North Sea (S.W.) and English Channel (E.).*

From the results so far obtained, it appears that spawning occurs chiefly west of 3° E. long., and at places where the bottom deposit contains an appreciably large proportion of stones and other coarse material. The period of spawning varies with the latitude, thus: Off Northumberland in August and September; on and around the south part of the Dogger and off Whitby in September and October; off the Wash and Norfolk coast in October and November; in the southern termination of the North Sea and in the eastern part of the English Channel from November to January. The post-larvæ, as they rise from the bottom and reach a certain size, are drifted towards the shore on both sides of the North Sea. From observations made in the Wash, Thames Estuary and adjacent waters, it appears that it may be possible to distinguish adults of the more northern autumn-spawning herring from those of the southern winter-spawning herring by the size of the area delimited by the first winter ring in their scales.

12. Mr. R. S. Clark.—*Features in the Development of Rays and Skates.*

The egg-capsules when deposited are buried in sand, and in most cases in shallow water. There they undergo a period of incubation, ranging in the different species from four to fifteen months. They are adapted to the life of the embryo. At first they are filled with a thick plug of albumen, which soon disappears or is absorbed, and then a constant stream of water passes through the open slits on the horns, the aëration being assisted by the rhythmical movement of the embryo. Transitory branchial filaments, elongations of the gill lamellæ, which are absent in the spiracle, persist to the end of embryonic life and are gradually absorbed with the development of the gill arches. Their function appears highly respiratory, but they may help in absorption, as may probably happen in the absorption of the albumen. Fertilisation of the egg must be effected in the upper reaches of the oviduct, and the subsequent passage of the complete egg and capsule must be fairly rapid. A single copulation has been found effective after a lapse of four months. The embryo becomes oriented towards the long horn end of the capsule, through which it passes head first at hatching. The yolk is withdrawn into an internal yolk sac which opens directly into the spiral valve. A considerable amount of yolk is retained

in this way for the needs of the young fish immediately after hatching. During the initial stages of free existence the young fish feeds chiefly on amphipods and small crangonids. Post-embryonic changes are well defined. The tip of the tail, behind the second dorsal fin, is gradually absorbed and becomes quite short in comparison with the embryonic elongation.

Specimens and photographs were exhibited of nine species, eight of which were secured at Plymouth and reared in the laboratory tanks. These included a complete developmental series of embryos, egg capsules, newly hatched young, and young fish up to a few months old.

Monday, September 11.

13. Joint Discussion with Section K on *The Present Position of Darwinism.* (See p. 399.)

14. Mr. A. M. Carr-Saunders.—*Problems of Geographical Distribution in Spitsbergen.*

15. Prof. A. Meek.—*The Fate of the Segmentation Cavity of the Frog's Egg.*

16. Prof. E. B. Poulton, F.R.S.—*Experimental Evidence for the Hereditary Transmission of Small Variations such as would be required to initiate a Mimetic Resemblance in Butterflies.*

The *sudden* evolution of a complex mimetic pattern, fully formed and complete, is difficult or even impossible to explain. We are compelled to believe that there has been a development in stages, each represented by a comparatively small variation. Certain authorities on heredity have maintained that such variations are not transmitted. An investigation designed to test this conclusion has been carried on with the common Currant Moth (*Abraxas grossulariata*), and it has been shown that the fusion of two black patches on the fore wing into a single bar, present in the female parent, was transmitted to nearly all of her offspring. Next year will probably decide whether the transmission followed Mendelian laws; but, Mendelian or non-Mendelian, the variation was certainly hereditary, and was of just such a kind and magnitude as would furnish one of the steps towards a complex mimetic pattern.

17. Mr. C. Tate Regan, F.R.S.—*Some Examples of Adaptative Evolution in Fishes.*

Three examples were given illustrating a marked adaptative evolution without any other change of structure. (1) *Cæcobarbus*, from a small subterranean lake in the Lower Congo, may be described as a *Barbus* that has lost its eyes and its pigment and has developed the lateral line system. (2) *Epibulus insidiata*, of the Indo-Pacific, may be described as a *Cheilinus* with an extremely protractile mouth; the quadrate bone is long and freely movable. (3) *Echeneis* is a *Percoid* with the spinous dorsal fin represented by a suctorial disc on the head, the fin spines being transverse laminæ. It may have originated from a pelagic fish with habits like the pilot fish (*Naucrates*).

18. A meeting took place at the Town Hall in Grimsby at 7.30 p.m., when Dr. E. J. Allen, F.R.S., and Prof. J. Stanley Gardiner, F.R.S., opened a discussion on *Scientific Fishery Research and the Fishing Industry.*

Tuesday, September 12.

19. Dr. W. R. G. Atkins.—*The Hydrogen ion Concentration of Soils and Natural Waters in Relation to Animal Distribution.*

It has been shown that a close connection exists between the distribution of plants and the soil reaction. Observations prove the relation of many animals, especially insects, to specific plants. Their distribution is, therefore, indirectly related to the soil reaction. A direct relation also exists. Certain earthworms are poisoned by the peaty soil of Dartmoor, and O. Arrhenius has shown that earthworms die outside certain limits of reaction. Snails, too, are similarly affected, *Helicella caperata* and *H. virgata* have never been found on acid soil. Among Crustacea, *Asellus aquaticus* lives in slightly acid water and *Gammarus pulex* in alkaline streams. In captivity *G. pulex* lives and breeds only if in water alkaline from calcium salts. MacGregor has shown how important the reaction of the water is for mosquito larvæ.

20. Miss K. Carpenter.—*Fresh Water Fauna of Aberystwyth Area in Relation to Lead Pollution.*

Faunistic study of the streams of this area reveals a general paucity referable to the scarcity of lime salts, but more remarkable is the almost complete barrenness of the larger rivers, Rheidol and Ystwyth. Out of a total of sixty Invertebrate species (Protozoa, Rotifera and Nematoda excepted) collected in local streams, only eleven occur in the main stream of the Rheidol and nine in the Ystwyth; further, these waters contain no fish. The Clarach, a smaller stream (length eight miles, Rheidol twenty-five, Ystwyth twenty-six and three-quarters), has nevertheless a fauna of thirty-eight Invertebrate species at one locality, and also contains trout. These last occur in several quite small brooks, many of which have a considerably longer fauna-list than the main rivers. The selective factor is undoubtedly the pollution of the rivers by contamination from mine-waters and refuse-heaps on the sites of old lead-workings in the hill regions; correlation of fauna-lists with chemical analyses of water and sediments establishes this beyond a doubt, but there remains the further question of the actual method of operation of this factor, whether through clogging of delicate organs by galena-grit or through infiltration of dissolved lead-compounds. On the former and generally favoured alternative are based the recommendations of the Ministry of Agriculture and Fisheries for safeguarding river fisheries against lead pollution, yet there is strong evidence of the working of the second factor in this district. The Clarach, like the Rheidol and Ystwyth, has mines near its headwaters, and the percentage of lead solid in river sediment is highest in the Clarach (0.12 per cent. Clarach, 0.085 per cent. Rheidol, 0.01 per cent. Ystwyth, in normal times), so that the comparative wealth of species in the river already noted can only be related to the absence of any appreciable quantity of dissolved lead substance, in contrast to the normal proportions of 0.02 and 0.04 mgr. per litre in Rheidol and Ystwyth respectively. Even if mechanical clogging of the gills occurs in fishes, recent observations definitely point to the solution factor as affecting the Invertebrate food organisms and the supporting flora. Further evidence of similar trend has been derived from observation of the peculiar conditions consequent upon last summer's drought.

21. Mr. Julian S. Huxley.—*Time-Relations in Amphibian Metamorphosis.*

Metamorphosis represents the point at which one phase changes into a second. The species in metamorphosing organisms is dimorphic, but the dimorphism is consecutive. An interesting parallel can be drawn with sexual dimorphism. In most animals sexual dimorphism is simultaneous, but in protandric and protogynous hermaphrodites it is consecutive. It is also consecutive in the 'intersexes' experimentally produced by Goldschmidt by crossing geographical varieties of Lymantria, and found in other forms by other workers. Consecutive sexual dimorphism is thus accompanied by what may be called a 'sex metamorphosis.' In all examples of consecutive dimorphism, whether the metamorphosis is from larva to adult or from one sex to the other, the morphological

change appears to be accompanied by a change in metabolism. This may be due to external factors, but in higher forms, as in Lymantria or Amphibia, depends mainly on internal factors. It can, however, be affected by external factors even in these forms. Examples are given of alterations of the time of metamorphosis by various factors. It is pointed out that by this means we can study the time-relations of developmental processes. For instance, the fact that Urodele larvæ kept at low temperatures reach a greater size before metamorphosing than do those kept at optimum temperatures indicates that cold decreases the activity of the processes leading to thyroid differentiation more than it does those leading to general growth of the rest of the larval organism. The failure of Perennibranchiates to metamorphose in spite of possessing active thyroids was discussed, and the idea of a balance between thyroid and the metabolism of other tissues of the larva, a balance which may be altered by changes in either member of the pair, was stressed.

22. Dr. F. A. E. Crew.—*Developmental Intersexuality in the Domesticated Mammals.*

Abnormality of the reproductive system taking the form of an intimate mixture of male and female structures is by no means rare in the domesticated mammals. Many cases in the horse, pig, goat, and cattle have been examined and in all the general condition is the same. It is reasonable, therefore, to conclude that the underlying cause is similar. The history of many such cases is that an individual, regarded as a female for the first year or so of its life, assumes certain characters of the male. The condition is best explained on the assumption that such individuals are males in which sex-differentiation has been much delayed. The process of differentiation consists of two stages: in the first, the gonads become differentiated; in the second, under the direction of the sex-hormone, the remaining structures of the sex-equipment are modelled to one of two plans, the male or the female. If the first stage is delayed, the sex-hormone is not exhibited, and in the absence of any specific control the Wolffian and Müllerian ducts pursue an equal and parallel development under the common stimulus of nutrition; the urogenital sinus with its genital tubercle becomes a well-grown cleft with a phallus in its ventral commissure. Later, when the gonads become differentiated, only those structures which are not too full-grown can respond to the stimulus of the sex-hormone. It is because the undifferentiated full-grown form is more closely mirrored by the immature female that the individual is regarded as a female.

23. Dr. A. Smith Woodward.—*Demonstration of a model of the Rhodesian Skull.*

24. Miss A. Dixon.—*The Periodicity in the Protozoan Fauna of a Pond.*

An investigation, by means of weekly collections of pond material, was made in a Manchester pond during thirty-four months. The results show that there is a marked periodicity in the freshwater protozoa, many showing a double rhythm, with the maxima in the autumn and spring months, which is shown by an increase of species, as well as an increase of individuals. Many of the bottom-loving species in the maximum periods spread to the mid and surface layers of the water, where they are found at no other times. There is less evidence of periodicity at the bottom, where many are continuous. Some of the smaller fluctuations in protozoan numbers appear to be due to the changes in the amount of sunshine, which, except for protozoa having chromatophores, has on the whole a depressing influence. The time for least activity in the protozoa fauna is in June, July, and December.

25. Dr. J. W. Munro.—*The Natural History of the Large Pine Weevil.*

The Large Pine Weevil—an important enemy of young forest plantations. General life-history and relation to the forest. Common control measures used

against it. Recent experimental work in Central Europe and in Britain. Improved control measures based on this work, with some account of progress.

26. Meeting with Hull Naturalists, when Prof. W. M. Tattersall gave *An Account of the Work of the Lancashire and Cheshire Fauna Committee.*

The work of this Committee, founded in 1914, is an attempt to enlist the co-operation of the local natural history societies and field clubs in the counties of Lancashire and Cheshire and to co-ordinate their work with a view to a complete faunal survey of the area. The societies are asked to collect material and forward it, with the necessary data, to the headquarters of the Committee at the Manchester Museum. From here it is sent out to referees and experts for identification, returned by them to headquarters, and redistributed to the societies concerned or to the local museums, as desired. The records are card-catalogued and published in the reports of the Committee. The card-catalogue is kept at headquarters and is available for reference to all workers in the area.

SECTION E.—GEOGRAPHY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 409.)

Thursday, September 7.

1. Miss E. C. Semple.—*The Influence of Geographic Conditions upon Ancient Mediterranean Agriculture.*

2. Mrs. H. Ormsby.—*The Danube as a Waterway.*

What is meant by a waterway?—The geographical conditions affecting the use of a river as a means of transport—To what extent these conditions are important in the case of the Danube—The traffic on the Danube before the War: why it was comparatively so insignificant—The future possibilities of the river as (*a*) a trans-Continental route, (*b*) for local traffic.

3. Presidential Address by Dr. Marion I. Newbigin on *Human Geography: First Principles and some Applications.* (See p. 94.)

4. Sir Philip Brocklehurst, Bart.—*Through Wadai.*

Friday, September 8.

5. Prof. J. F. Unstead.—*The Belt of Political Change in Europe.*

The 'New' States of Europe form a narrow but continuous belt separating Western Europe from the Russian region—This belt is in several respects transitional between West and East, but has certain characteristics peculiar to it—The consequences of its situation between the maritime West, with a complicated geological history, and the Continental East, with a less disturbed geological past, are traced in its present ethnic conditions and its social and political problems.

6. Mr. Ll. Rodwell Jones.—*The Port of Hull: a Geographical Study of Port Development.*

Geographical factors concerned in the growth of a port are of two kinds, (*a*) those pertaining to the general hinterland, (*b*) more local factors—These considerations applied to Hull—The Ouse river system in its relation to Hull—

Character of early trade of Hull—Artificial extension of the river system—The Yorkshire, Derbyshire and Notts coalfield of 1830—The trade of the modern port as influenced by its general environment, with special reference to the localisation of fish traffic, oilseeds, and coal export.

7. Mr. C. Midgley.—*Holderness: some aspects of Water Supply as a Geographical Factor.*

Two contrasted regions, chalk and clay—The significance of this in water supply and consequent human values—Different lines of development and economic histories—The reflexion of this in the past and present distribution of population—Local prehistoric settlements and their relation to water supply—Evidence of Domesday—Distribution of mediæval religious houses—Population at the beginning and end of the nineteenth century—Water supply of Hull.

8. Mr. H. M. Spink.—*Some Geographical Aspects of Recent Developments of Water-power.*

General summary of the distribution of potential water-powers—Recent developments in North America, Europe, and Japan—The great importance of tropical Africa as a source of power—Hydro-electric power and the location of industries—Probable effect upon the distribution of population—Importance of improvements in long-distance transmission—Importance of water-power and steam-power in combination—Schemes in America on these lines—The 'Super-Power' Zone.

9. Mr. A. V. Williamson.—*Irrigation in the Indo-Gangetic Alluvium.*

The need for irrigation—Geographical factors governing the methods adopted to meet the need—Irrigation works, (*a*) wells, (*b*) canals—Efficiency of irrigation—Agricultural aspects.

10. Mr. D. C. T. Mekie.—*The Trend of World Commerce.*

Trade of early times determined by differences in productions of different climatic zones—This trade was almost entirely north and south, and largely in luxuries—Likewise, trade of Europe during Middle Ages and English trade down to nineteenth century was north-south trade—Change due to industrial revolution—Examination of Russell Smith's laws of trade: 'North-south trade is the trade of the future—at present the great bulk of our commerce is east-west trade.'

Monday, September 11.

11. Dr. T. Ashby.—*Early Maps of Malta.*

Maps of sixteenth, seventeenth, eighteenth, and nineteenth centuries of the whole Island and Valetta.

12. Mr. R. A. Frazer.—*Topographical Work in Spitsbergen.*

Region under investigation (Garwoodland or Bünsowland): main topographical features of the country north-east of Ice Fojrd—Résumé of previous exploration—Objects of the expedition—Mount Terrier and the Nordenskjöld glacier—Notes on the high interior—Exploration between the Mount Newton and Mount Svanberg areas—Problems still outstanding.

13. Mr. F. Debenham, O.B.E.—*Survey in Polar Regions.*

A description of land surveys in the Polar regions, with special reference to the Antarctic—Types of surveys required for Polar regions—Past methods described and discussed—The problem of a short season and adverse weather conditions as it affects the general methods, and also the types of instruments—Suggestions for future work.

14. Mr. A. G. Ogilvie, O.B.E.—*The Mapping of Latin America.*

15. Discussion on the *Use of Mercator's Projection for Air-maps.* Opener: Col. E. M. Jack, C.M.G., D.S.O.

16. Prof. P. M. Roxby.—*Peking: its place in the life of modern China.*

The rôle of Peking in the history of China—External features of the city as reflecting old and new forces in the life of China—The social life of modern Peking—Relation of East and West in the city—A meeting-place of cultural influences—The National University and the Chinese renascence—Peking as the national capital—A discussion of the arguments for and against the removal of the central Government from Peking.

17. Joint Discussion with Sections C and H on *The Relation of Early Man to Phases of the Ice Age in Britain.* Opener: Mr. H. J. E. Peake.

Tuesday, September 12.

18. Dr. Vaughan Cornish.—*The Isothermal Frontier of Ancient Cities.*

The northern frontiers of the great empires of ancient history were consecutive from the North Sea to the Sea of Japan. The author has examined the position of this line of separation between city life and that of forest and prairie people at the beginning of the second century A.D., not long before the great barbarian irruption, and finds that the present actual mean annual temperature is, with little variation, the same throughout the entire length of about seven thousand miles. The paper contains a table of the recorded temperatures.

19. Mr. R. R. Walls.—*Portuguese Nyasaland: its Geographical Problems.*

Geological structure and geographical features—Climatic conditions—The Great Rift valley in Portuguese Nyasaland—The drying up of East Africa and its bearing on development—The population problem.

20. Miss H. A. Wilcox.—*A Scheme for the Preparation of a Map of the Early Woodlands of Britain.*

Early woodlands in particular of importance for the interpretation of some other distributions—Construction of map: physical and historical evidence—Illustrations of maps prepared for South-West and South-East England—Uses of the map.

21. Joint Discussion with Section A (Cosmical Physics Sub-section) on *Monsoons.* Opener: Dr. G. C. Simpson, C.B.E., F.R.S.

EXHIBITS DURING THE MEETING: MAPS OF HULL.

A collection of maps of Hull and the Humber estuary, formed by Mr. T. Sheppard, was on view during the meeting.

SECTION F.
ECONOMIC SCIENCE AND STATISTICS.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 409.)

Thursday, September 7.

1. Mr. J. L. Cohen.—*The Future of Unemployment Insurance.*

(i) The fundamental principles underlying the British scheme of unemployment insurance are sound.

The inadequate application of those principles has produced the present discontent.

An examination of the errors of the past.

Current misleading conceptions relating to 'employment exchanges,' 'doles,' the 'bankruptcy' of the Unemployment Insurance Fund.

(ii) Shall industries bear the costs of maintaining their own unemployed? The growth of this idea.

The rota system of the Cotton Control Board; the Dockers' Scheme; the Building Guilds; Establishment Funds.

Special schemes under the British Act.

The case for and against.

(iii) Can unemployment insurance be so organised as to reduce the amount of unemployment? The Wisconsin Bill for 'Unemployment Compensation and Prevention.' The proposal examined.

(iv) The need for a committee of inquiry into :

(i) The possible elimination of the trade cycle.

(ii) The advisability of unemployment insurance organised by trades.

(iii) The possibility of a unified centralised system of social insurance.

2. Prof. J. G. Smith.—*Modern Municipal Markets and their Economic Significance.*

Market organisations for the handling of perishable produce, municipal and non-municipal, British and foreign, were briefly reviewed, and the conclusion was reached that markets under the direct management of local authorities are to be preferred to the system now general in this country, under which municipal authorities hire out stalls and sites in publicly owned buildings and/or levy tolls on all produce dealt in. Wholesale and retail markets in this respect present the same problems. Hitherto attempts at organisation by producers to obtain better prices, and by consumers to effect reduction in city prices, have met with failure; and co-operative societies do not succeed in eliminating the middlemen, who intercept so large a part of the price paid by the consumer. Decentralisation, through municipalisation of wholesale marketing, would save much of the expense that is now incurred in unnecessary transportation and prevent, for example, produce grown in the Evesham district, destined, perhaps, for consumption ultimately in Birmingham, from being forwarded to Covent Garden for sale there to Midland buyers. Moreover, such decentralisation would not result in lessened prices for producers.

3. Joint Discussion with Sections A and M on *Weather Cycles in Relation to Agriculture and Industrial Fluctuations.* Opener: Sir W. Beveridge, K.C.B.

Friday, September 8.

4. Mr. R. B. Forrester.—*The Measurement of Productivity.*

International comparisons of productive efficiency in industry and agriculture. Survey of the tests which have been used (*a*) in comparing British and foreign agriculture by Sir Thomas Middleton, Mr. Ashby, and other writers; (*b*) in contrasting the position of the cotton industry in different countries by

Schulze-Goevernitz Aftalion, Copeland, and later students; (c) in comparing British and American industry by means of the Census of Manufactures and the Census of Production. Difficulties involved in employing these measures to gauge the relative position of an industry in different countries, or to judge of the varying efficiency of the factors of production in each case. The influence of industrial and agrarian policy, the scale of production, and other causes, upon the relative position of an industry in different countries.

5. Prof. A. L. Bowley.—*A Comparison of Wholesale and Retail Prices Since the War.*

Three pairs of series are taken for the thirty-seven months, April 1919 to April 1922: (*a*) Retail food-index and the 'Statist' wholesale food-index; (*b*) retail food-index with four seasonal commodities removed and the wholesale food-index; (*c*) cost-of-living index and the 'Statist' general index. A number of methods having been tried for finding a formula connecting retail and wholesale prices, it is found that in (*a*) and (*b*) the connection is closer between retail prices and wholesale three months earlier than with any other time lag, while in (*c*) a four months' lag gives the best result.

If p is written for the retail price-index in any month, p_{-1} p_{-2} . . . for retail price-indices in the two previous months, and P_{-2} P_{-3} . . . for wholesale indices similarly, the best equations found are—

(*a*) $p = 50.5 + .524P_{-3} + .213p_{-2}$ (6.1),
(*b*) $p = 38 + .497P_{-3} + .28p_{-2}$ (6.9),
(*c*) $p = 102.4 + .494P_{-4}$ (5.9),

where the mean differences between the formula and the actual records are given in brackets.

If two exceptional periods of four months each are removed a formula for (c) ($p=103.6+.476P_{-4}$) gives a mean difference for the remaining months of only 3.6 points, or 1.7 per cent., and the coefficient of correlation is .991. In each case the pre-War index is taken as 100.

6. Mr. W. Hamilton Whyte.—*The War and Stock Markets.*

Conditions governing stock markets before the War. Different types of transactions described, including system of 'carry over.' Outbreak of hostilities and closing of Stock Exchange. House reopened under special restrictions by which speculative dealings were prohibited. Abolition of carry-over system had three results: (1) Increase of loan accounts with banks against stock purchases. (2) Increase of banks' influence over markets. (3) Less critical view of the influence of the 'bear' on stability of markets. Criticism and defence of banks' policy during the rise and fall. Effects of the War on stockbroking twofold: (1) Changed the structure of the business; (2) diverted capital from fixed-interest-bearing securities into more speculative stocks. This shows a change of policy on the part of the pure investor. A perusal of market values from 1914-20 suggests (1) that there has been a steady fall in price of fixed-interest-bearing securities concurrently with extensive Government borrowings; (2) the application of such loans to war production gave security to certain classes of speculative stocks which enhanced their market values. This condition only temporary. Need for higher standard of business training and greater co-operation between universities and the business world.

Monday, September 11.

7. **Presidential Address** by Prof. F. J. Edgeworth on *Equal Pay to Men and Women for Equal Work.* (See p. 106.)

8. Mr. C. F. Bickerdike.—*The Question of the Possibility of Controlling Industrial Fluctuations.*

The paper related to normal circumstances of peace-time. Something has to be said about causes of fluctuations. Weather cycles may affect the dates

when changes occur, but there would be fluctuations regardless of such influences and due to reasons not obviously beyond human control. The important question is whether the working of the monetary system is the principal cause of fluctuations. Outline of the views of Irving Fisher and Hawtrey. Criticism of the purely monetary theories. Influence of the durability of capital goods. Monetary theory suggests that there should be a simple solution in better control of Bank rate. Some reasons for questioning whether this can be made effective. Even if we disregard the multiplicity of nations with independent banking systems, it is questioned whether control of Bank rate for the purpose of stabilising industry could be successful in more than a moderate degree; but though there may not be any quite simple method of control, probably a more complex method could be developed in time. When international co-operation has to be considered success is more problematic. We may look forward, however, to a gradual improvement through a more complete understanding of the working of the monetary system, but some departure from the simple gold standard may be necessary. Merely to aim at long-period stabilisation on the lines of Irving Fisher's proposals, however, does not go far towards checking ordinary fluctuations of good and bad trade.

9. Miss A. Ashley.—*The English and Scottish Poor Law in Relation to the Able-bodied.*

Contrast in principle between English and Scottish Poor Law with regard to responsibility for the able-bodied and their families until the Poor Law Emergency Provisions (Scotland) Act, 1921. This partly the outcome of different history of the Poor Law in England and Scotland. Difference in actual practice less than in theory, because :—

(1) The general use of the 'workhouse test' for the able-bodied in England kept many from applying to the Poor Law.

(2) The requirement that a man must be disabled to be relieved was often laxly interpreted in Scotland, and the argument that if a destitute man is able-bodied he will not, if unrelieved, remain so long sometimes used.

The difference in principle involved, however, the absence of all special provision for the able-bodied in Scotland, such as casual wards and special forms of employment for able-bodied inmates of Poor-houses.

Underlying assumption of much of the policy of both countries the receipt of help from Poor Law shameful, and the number of legal paupers to be kept low, not only for economy, but for the moral good of the people.

The individualistic doctrine underlying this view largely abandoned, but until 1921 with the effect not of removing the stigma from the receipt of Poor relief, but of multiplying separate pieces of machinery to administer forms of public support not held to be shameful (*e.g.* Old-age Pensions).

Result of War and of period of extreme depression after the War :

(1) To multiply the cases and emphasise the existence of prolonged unemployment unconnected with personal fault.

(2) To increase the forms of public support outside the Poor Law (pensions, unemployment donation, special provision for school-children, for infants, &c.).

(3) These having proved inadequate, to break down the practice of refusing Poor-Law out-relief to the able-bodied (and in Scotland to make the relief of the able-bodied legal, when it had already become common under the Act of 1921).

(4) To lessen and almost abolish the feeling that help from the Poor Law is more shameful than other forms of relief.

Thus the inconvenience and undesirability of a large variety of agencies is being demonstrated, while at the same time the objection to the old single agency for the relief of distress is largely removed.

The position may be seen by examining recent developments in sample English and Scottish towns, with details of typical cases.

Possible future developments and conclusions.

Tuesday, September 12.

10. Joint Discussion with Section M on *The Possibility of Increasing the Food Supply of the Nation.* Opener: Sir John Russell, F.R.S.

11. Miss H. Reynard.—*Human Motive in Industry.*

The need of an adequate appreciation of the motives underlying our industrial organisation. The machinery which served in the past and might still be best adapted to the satisfaction of our material needs will not work if human motive ceases to supply the power. The psychology of the business man. The strength and weakness of his position. The development of joint-stock enterprise has surrendered one of the most important positions of capitalistic institutions. The psychology of the worker. His attitude towards (*a*) Payment by result; (*b*) the making of profits; (*c*) the control of industry. The necessity for finding a *modus vivendi.* The two essential questions to which an answer must be found : (1) What is a reasonable profit? (2) What measure of control ought the worker to have?

Wednesday, September 13.

12. Mr. J. E. Allen.—*Report of Committee on Credit, Currency, and Finance.* (See p. 319.)

SECTION G.—ENGINEERING.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 409.)

Thursday, September 7.

1. Discussion on *The Strength of Railway Bridges.*

(*a*) Mr. J. S. Wilson.—*Introductory Statement.*

(*b*) Mr. A. G. Cookson and Mr. J. S. Nicholas.—*The Strength of Railway Bridges, with special reference to the proposals of the Ministry of Transport.*

The Ministry of Transport have concluded, as a result of certain tests carried out by their officers, that on many British railways insufficient provision has been made for the effect of the live load. It is suggested in this paper that their conclusions are not correct interpretations of their observations, and that those tests, as well as many others carried out elsewhere, prove beyond doubt that bridgework in this country is much stronger than is generally believed. The authors confirm their opinion by reference to certain old structures still carrying main-line traffic without restriction; calculations and actual stress measurements are given. Important discrepancies between calculated and observed stresses are pointed out, and the authors emphasise the view that the life and endurance of existing bridges as determined by experienced maintenance engineers is the only reliable criterion. Rules based on sixty years' maintenance experience are suggested.

(*c*) Mr. Conrad Gribble.—*Present-day Problems and Tendencies in Railway Bridge Design.*

Desire for economy through (*a*) durability, (*b*) accuracy of design—Difference between bridge problems in Great Britain and abroad, our work being principally maintenance and renewal—Increased use of concrete—Suggested employment of high-tensile alloy steels—Possibility of stainless steel for bridges—' Cement-gun ' protection for steelwork—Problem of secondary stresses—Research work on bridges—Need for information as to (*a*) ultimate resistance of girders, (*b*) precise

amount of stress per unit load and distribution of stress through structure—Suggested testing to destruction of full-size bridges.

Rivets.—Need for information as to (*a*) ultimate strength of rivet, (*b*) distribution of stress in riveted girders. Bridge Problems arising out of railway amalgamations.

(*d*) Mr. J. S. Wilson and Prof. B. P. Haigh.—*The Influence of Rivet-holes on the Strength and Endurance of Steel Structures.*

The numerous rivet-holes that pierce the plates and rolled sections used in bridges and other steel structures reduce the strength and endurance of tension members, particularly under varying loads. The arbitrary allowances which are made for this reduction are discussed in relation to the results of a number of 'fatigue' tests carried out by the authors.

Small plates with different spacings of rivet-holes, tested in a Haigh fatigue-testing machine adjusted to apply tensile stresses varying from a minimum to a maximum 2,000 times per minute, are shown to crack through the holes in directions perpendicular to the axis of the applied pull—not necessarily across the shortest alternative path between the holes. The results are compared with theoretical deductions from established general principles.

The fatigue tests are supplemented with steady-load extensometer tests on plates with different rivet-hole spacings.

In the afternoon the Canister Works of Messrs. Reckitt & Sons, Ltd., were visited.

Friday, September 8.

2. Mr. G. V. Maxted.—*The Equipment of a modern Cement Works, with special reference to the work of the Humber Portland Cement Co.*

The works were visited in the afternoon.

3. Prof. F. C. Lea, O.B.E., and Mr. R. E. Stradling.—*The Resistance to Fire of Concrete and Reinforced Concrete.*

The paper describes a series of experiments carried out in the Department of Civil Engineering of the University of Birmingham, with the object of investigating the effect of high temperature on Portland cement concrete, plain and reinforced.

It is shown that concretes as ordinarily used in practice to-day, containing quartz sand as a fine aggregate, lose about 20 per cent. of their strength when heated up to a temperature of 550° C. Above this temperature the loss in strength is very much greater, and at about 700° C. the loss is of the order of 70 per cent. to 80 per cent. It is suggested that this is probably due to the expansion of the quartz at 575° C., when the α-β transformation takes place.

Much more fire-resistant concretes can be made by using a fine aggregate made of brick or natural rock (such as basalts or dolerites), which do not contain free quartz in any large quantities. The loss with such a material is only of the order of 30-40 per cent. at 700° C. and even higher (1000° C.).

It is pointed out, however, that although these special concretes may carry their load during a fire, yet the after-effects may be sufficient to cause failure, and the reasons for this are discussed.

Experiments are also described to obtain data on the linear expansion of Portland cement when exposed to temperatures up to 800° C. The data obtained indicate the probable cause of the spalling off of concrete surrounding steel reinforcement.

The suggestion is made that the concrete as normally used in reinforced concrete is not an effective fire-resisting material. It will spall off and expose the steel during the fire, and if the building does not then fail, the moisture from the atmosphere finds access to the dehydrated lime, causing cracking and disintegration of the concrete that has been heated above a certain temperature.

4. Presidential Address, by Prof. T. Hudson Beare, on *Some Australian Railway Problems.* (See p. 133.)

Saturday, September 9.

The Hull Docks were visited to inspect features of special engineering interest.

Monday, September 11.

5. Discussion on *Economic Steam Production, with special reference to Marine Practice.*

(*a*) Dr. C. H. Lander.—*Home-produced Oil Fuel.*

During the past fifteen years a complete substitution of coal by oil fuel in the Navy has been effected. In the Mercantile Marine development has taken place at a slower rate, but during the year 1920-21 58 per cent. of the new vessels classed under Lloyd's Register were fitted for burning oil fuel.

The question of home sources of supply has been under consideration since 1912, but since then the urgency of the problem has increased.

The natural source of oil fuel of this country is discussed, with especial reference to distillation of oil, and it is shown that the only practicable method is the replacement of coal used in the domestic grate and in industry by some form of manufactured smokeless fuel.

Figures of possible oil production are given, based on the experimental work on coal carried out by the Fuel Research Board, and the suitability of this oil for steam-raising is discussed.

(*b*) Engr. Comdr. Fraser Shaw, R.N.

Two distinct questions: commercial economy, fuel economy. Commercial economy always wins the argument.

With scientific aids the economy of a well-run installation on land does not leave very much to be desired. Considerable drop in efficiency between well-run and badly run plants. Avoidable losses in latter due to leaky settings and inferior personnel (supervision and stoking).

Considerations confined to coal-fired single- and double-ended return tube tank boilers. Marine boiler at a disadvantage compared with land boiler, owing to cramped space and reduced heating surface for given output; but at an advantage owing to being self-contained and not dependent on brick settings.

By the use of liquid fuel stokehold personnel may be reduced and its efficiency increased.

Distinct improvement possible even in coal-fired ships by introduction of scientific instruments, so far little used at sea.

Superheaters in fairly general use at sea.

Economisers not used at sea so much as on land.

In considerations of economy first place must be assigned to system of control to stop gross waste and work existing appliances and known methods to best advantage.

(*c*) Engr. Comdr. R. Beeman, C.M.G., R.N.

The question of economic steam production is dealt with from the naval aspect.

Improvement in economy of fuel leads in any specific case to a greater radius of action or, on the same radius of action, to a lesser weight of fuel to be carried, and consequently a higher speed, both important advantages. Economy, however, depends, given equal conditions of running, upon the initial design, which in turn depends in the main upon the weight and space that can be given to the machinery. Any design is thus a compromise between the several leading

offensive and defensive characteristics of a war-vessel, and any one of these characteristics, such, for example, as economy in fuel, has on occasion to give way to other more important features.

The details of naval practice in respect to the burning of fuel are described, together with some of the results obtained, the types of machinery fitted, and the measures adopted to attain and maintain fuel economy on service.

(*d*) Mr. A. Spyer.

The works of the National Radiator Co. were visited.

Tuesday, September 12.

6. Mr. J. Richardson.—*The Propelling Machinery of the Cargo-Carrier of the Future.*

7. Joint Discussion with Section L on *The Effect of Reformed Methods in Teaching Mathematics.* (See p. 406.)

The works of the British Oil and Cake Mills, Ltd., were visited.

Wednesday, September 13.

8. Dr. E. O. Turner.—*Electrical Ignition Apparatus for Internal Combustion Engines.*

(1) *General.*—The requirements to be fulfilled by ignition apparatus were stated; the importance of sparking being continuous with high conductivity secondary branch circuit was pointed out, and the term 'utility' explained. The effect of varying cylinder compression due to varying speed and firing angle was considered, and in order to provide a means of comparing ignition units of different design expressions were suggested as figures of merit for induction coils and magnetos respectively. The use of tungsten and iridio-platinum alloys for contact-breakers was referred to, and the question of the time of duration of contact at high speed and its influence on functioning was examined.

(2) *Ignition by Induction Coil.*—The magnetic circuit, primary and secondary windings, and operation under the conditions obtaining in practice were discussed, and curves showing the results of experiments on induction coils designed for use with automobiles and with high-compression stationary gas-engines were illustrated.

(3) *Ignition by Magneto.*—The magneto was considered in two aspects—as a constant flux alternator on short circuit for the first part of each cycle (until the contacts open), and thereafter as an induction coil breaking the current just established. Expressions for the equivalent magnetic circuit and the 'spark flux' were deduced; the latter was shown to be dependent on the short-circuit current and the equivalent permeance of the magneto. Drawbacks of the tungsten steel magnet magneto were pointed out, and the improvements made possible by the substitution of cobalt steel magnets were referred to; typical experimental curves supporting the conclusions arrived at were shown.

9. Mr. C. E. Stromeyer.—*Resolution of Compound Stresses.*

Stresses are always balanced; the push and pull in a bar are the same at both its ends. Forces, on the other hand, when acting on masses may be considered to be unbalanced. Nevertheless, in spite of this dissimilarity, the process of resolving stresses is based on the law of acceleration of masses. The more straightforward procedure would be to deal with the problem direct. Definition: A resultant force produces the same acceleration in a mass as is

produced by the forces which it replaces. Similarly, resultant stresses produce the same effects (strains) which are produced by the stresses which they replace. This idea is carried through in the present paper. A series of points in a solid are fixed upon and expressions set up which determine the relative displacements of these points if subjected to a number of stresses acting in various directions. These expressions contain terms which depend only on the intensities of the stresses and their directions, and not on the relative position of the selected points. These terms must be fulfilled by the resultant stresses, and thus a number of formulæ are obtained which determine the intensities and directions of the resultant stresses. Vector co-ordinates are employed. For stresses acting parallel to a single plane the simple rule is evolved that, adopting an arbitrary direction, all the angles which the stresses make with this direction must be doubled, and a polygon of stresses is then constructed with these doubled angles. The end-points of this polygon may be looked upon as being the foci of an ellipse or of a hyperbola, whose major axes are equal to the algebraic sum of the stresses. The direction has to be halved. This result can, of course, be obtained from the formulæ evolved with the help of the law of acceleration. The paper also deals with the resultants of stresses in space. The paper explains the use which can be made of this method of resolving stresses for measuring the strains in celluloid which are revealed by polarised light.

10. Mr. R. S. Whipple.—*Demonstration of the Collins Micro-Indicator for high-speed Engines.*

SECTION H.—ANTHROPOLOGY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 409.)

Thursday, September 7.

1. Presidential Address by Mr. H. J. E. Peake on *The Study of Man.* (See p. 150.)

2. Dr. Cyril Fox.—*The Distribution of Population in the Cambridge Region in Early Times, with special reference to the Bronze Age.*

The distribution in Britain of constructions attributable to the Neolithic and Early Bronze Ages suggests that the population was then limited to those areas, mainly upland, which must have been, under natural conditions, largely free from forest.

A topographical analysis of finds and remains of all culture periods from the Neolithic to the Saxon in a limited area—the Cambridge Region—was undertaken to determine whether this limitation was complete or partial, and when the clearing and occupation of forest areas commenced. The Cambridge Region is very suitable for the inquiry, since it possesses a wide range of soils and has yielded numerous finds of all periods.

The maps exhibited suggest (1) that the chalk belt and the eastern shoreline of the Fens were occupied from Neolithic times onwards; (2) that there was a gradual shift of population from N.E. to S.W., *i.e.* from the West Suffolk heathland to the fertile lands of the upper Cam and Ouse valleys, as agriculture developed; and (3) that the forest uplands were almost entirely unoccupied until the Roman period.

The distribution of population in the Bronze Age is, generally speaking, of a character intermediate between that of the Age which preceded it and that which followed, but it presents features of special interest.

3. Prof. W. J. Sollas, F.R.S.—*A Method in Comparative Craniometry and its Application to Homo Neanderthalensis.*

4. M. le Comte de St. Périer.—*The Unio and Anodonta in the Prehistoric Stations.*

5. Dr. T. Ashby.—*Recent Archæological Discoveries in Italy.*

The past year has not produced any sensational discoveries in Rome itself, though considerable excitement has been caused in England by the somewhat indiscreet announcement that the portraits of Peter and Paul in the hypogeum in Viale Manzoni (described at the Edinburgh Meeting) were contemporary. The tomb itself has been the subject of further investigation, and is now generally admitted to be Christian and to belong to the early third century after Christ.

In the neighbourhood of Rome, Ostia continues to provide features of interest, and another large house with a large central arcaded courtyard has recently been cleared. An important article on the Capitolium at Lanuvium makes it clear that the famous temple of Juno Sospita has not yet been brought to light, and that the only sanctuary so far found on the Acropolis (in 1914-15) is probably that of the Capitoline Triad. Important discoveries have also been made on the site of Horace's Sabine farm, where a large villa with fine decorative mosaic pavements (a small fragment of which has been known for over 100 years) has been brought to light.

Attention should be called to the initiation of the archæological map of Italy, with a careful survey of the neighbourhood of Terracina. Dr. Ashby and Mr. R. A. L. Fell, student of the British School at Rome, have followed the whole course of the Via Flaminia from Rome to Rimini. This road was the most important land route to North Italy and to the rest of Europe, though the remains of it are not so well known as they deserve to be. A very fine viaduct in particular, in the neighbourhood of Civita Castellana, the ancient Falerii Veteres, is almost entirely unknown.

6. Mr. S. Casson.—*Recent Archæological Discoveries in Athens.*

Three remarkable bases, one painted, two sculptured, have been discovered, which have furnished information on non-Olympian games not hitherto known.

Friday, September 8.

7. Mr. J. Whatmough.—*Inscribed Fragments of Stagshorn from North Italy.*

The accidental find (in November 1912), by a workman, of four pieces of inscribed stagshorn amongst débris which had slipped down into a quarry from the summit of a hill above Magrè (near Schio, twenty miles north-west of Vicenza, in one of the valleys leading up to the Brenner Pass), led to excavations which revealed evidences of a pre-Roman occupation, with a temple or sanctuary on the hilltop. Nearly a score of other inscribed fragments, also of stagshorn, were discovered, and are now preserved with the first four and the other objects found on the same site in the Museo Nazionale at Este, where I read the inscriptions in March 1922. The character of the other remains serves to date the sanctuary; they are of the same types as those of the fourth Este period.

The *alphabet* of the inscriptions is almost, though not quite, identical with that of the famous Venetic inscriptions of the Fondo Baratela (Este), and is clearly derived (like the Venetic) from the N. Etruscan alphabet. The *language*, however, is not Venetic nor Etruscan; it has certain features which suggest that it may be Indo-European. If it is rightly ascribed to 'Rhaetic' (known from inscriptions previously discovered), it would go to show that

this, too, was perhaps after all Indo-European rather than Etruscan—the speech, that is, of some early west Indo-European people isolated amongst Gauls and Etruscans—another blow to the theory which found the original home of the Etruscan race amongst the Rhaeti. The inscribed horns must be votive offerings, but it is not clear what were the features of the cult. The deity to whom the offerings were made, it has been suggested, was of the Artemis-Diana huntress type. This suggestion would strengthen still further the accumulating evidence of the northern origin of Artemis.

8. Mr. S. Casson.—*Recent Excavations in Macedonia.*

9. Dr. T. Ashby.—*Supplementary Excavations at Hal-Tarxien, Malta.*

The megalithic ruins of Hal-Tarxien, in Malta, have been recently excavated by Professor T. Zammit, and fully described by him in *Archaeologia.*[1] They consist of a large sanctuary of three different periods, with smaller rooms, probably priests' dwellings, annexed. Supplementary investigations were carried on under the floors by Dr. Thomas Ashby, at Professor Zammit's invitation in the spring of 1921, which bore out Professor Zammit's conclusions as to the relative age of the various portions of the building. In almost every part of the building which belonged to the first or second period an earlier floor was found below that which had previously been cleared, and under both floors a considerable quantity of pottery was found. In one case the removal of the floor (which, as in most cases, was of beaten limestone dust, known locally as *torba*) led to the discovery of a circular opening in the natural rock, closed by a round slab 2 ft. in diameter. When this was removed a cavity resembling a Benedictine bottle in shape was revealed. It was a little over 5 ft. in height and 3 ft. in diameter, and probably intended for the storage of grain, but only a quarter of an inch of earth was found in it.

In the portions of the building belonging to the third period there was, as a rule, no evidence of the existence of any earlier floor.

Monday, September 11.

10. Mr. E. K. Tratman.—*Explorations of Read's Cavern, near Burrington Combe, Somerset.*

The cave consists of a long rift chamber with offshoots, and lies at the junction of the limestone shales and the 'Z beds' of the mountain limestone.

The floor consists of a black hearth level 3 in. to 8 in. in depth, and covered, where possible, by a layer of stalagmite $\frac{1}{4}$ in. to 3 in. thick. Below is a layer of clay and boulders, never more than 2 ft. thick, barren of human remains.

All the remains, with a few exceptions, have been found in the hearth level, while the individual hearths are scattered throughout the cave, but one area appears to have been the main occupation area. Human bones are scanty as yet. Animal remains are abundant, and include sheep, pig, ox, horse, dog, and goat, as well as roedeer, wild cat, and wild boar.

The artefacts include articles of iron, bronze, bone, and stone and pottery. This last is usually fragmentary, but can be pieced together, and bears the designs typical of the period. The stone implements comprise spindle whorls and the upper stone of a saddle-backed quern. Needles and weaving implements of bone are found, as well as 'cheek pieces' made from antler. The artefacts of bronze include brooches, nave hoops, and rings, while amongst the iron are slave shackles, keys, nails, &c. The only weapons found so far are a spear-head, part of a lance, and, possibly, part of a dagger.

[1] lxvii., 127; lxviii., 263; lxx., 179

11. Mr. J. A. Davies.—*Exploration of Aveline's Hole, Burrington Combe, Somerset.*

The cave is a partially choked rift in the mountain limestone of the Mendips. It was first discovered in 1797, and excavation was carried out by several investigators, but all material discovered was lost.

The Spelæological Society of the University of Bristol commenced work in the cave in 1919. The floor was removed in layers of 1 ft. and sorted carefully outside. A depth of 3 ft. has been reached in this manner.

The finds include three dolichocephalic human skulls with broad faces, and many other human bones. These are considered to be remains of a modified Crô-Magnon people.

The artefacts discovered are many worked flints belonging to a Late Aurignacian or Early Tardenoisian industry, several bone implements, and a stag-horn harpoon with six barbs of the period Magdalenian 6 b. (l'Abbé Breuil).

Remains of about forty species of animals have been identified up to the present, including reindeer, lemming, brown bear, and lynx. The fauna is typical of the sub-Arctic forest. Below the cave earth containing these animal remains is a deep layer of loess which has yet to be explored. The cave earth and loess apparently represent two phases of the period lying between the Bühl and Geschnitz-Daun glaciations.

12. Miss Nina F. Layard.—*Prehistoric Cooking-places.*

The series of permanent prehistoric cooking-places which I am now excavating under the auspices of the Percy Sladen Trust were discovered by me in May 1921 at Buckenham Tofts Park, Norfolk. They consist of thousands of burnt flints reduced to a crackled condition. These flints were apparently used as heaters, for boiling water in vessels that would not stand the fire. They are found a foot or two beneath the sod, and invariably a few yards from a stream.

The bones and teeth of animals such as ox or horse, which occur between the heaters and the stream, point to the exact spots where the cooking took place. Either a wooden trough or a stretched hide would in all probability be the utensil used. This was filled with water, and red-hot flints were then shovelled into the vessel, when the water would soon be brought to the boil. The most important part of the discovery is the presence of numerous flint flakes and definite implements embedded in the burnt flint heaps and strewn around the cooking site. So far these seem to point to the Early Bronze Age, though a few fine specimens of Neolithic implements appear to have been found and used by the later comers.

13. Rt. Hon. Lord Dunsany.—*Worked Flints from the Sahara.*

14. Mr. H. W. Seton-Karr.—*Some Ancient Implements found in Some Desert Places of North Africa.*

15. Joint Discussion with Sections C and E on *The Relation of Early Man to Phases of the Ice Age in Britain.* Opener: Mr. H. J. E. Peake.

Tuesday, September 12.

16. Joint Discussion with Section J on *Mental Characters and Race.* Opener: Prof. J. L. Myres.

17. Mr. E. Torday.—*The Mutability of Custom among Congo Tribes.*

Among the tribes visited in the Belgian Congo remarkable cases of conservatism have to be recorded, as, for instance, the refusal of some Bushongo

to learn the craft of the potter, or the use of wooden hoes by the Batetela, the neighbours of such famous smiths as the Basonge. On the other hand there is a constant exchange of customs by neighbouring tribes: dress, dwellings, weapons, crafts, laws of inheritance, etc., fall under this heading. A section of the Babunda have abandoned cannibalism, independently from European influence, while the Northern Bambala have recently taken to it. The same people have given up the practice of circumcision. The sporadic appearance of coil baskets among the Baguana, the crossbow (as a toy) among Bambala, and firemaking by the groove method among the Tophoke have to be recorded.

In the interest of science, as well as of the reputation of the traveller, it will be necessary to record in the future more carefully than in the past the exact locality of observations. The question arises whether these changes are not contributory to the disintegration of tribes, as among the Bahuana, the Bapende, the Bambala, and others.

18. Dr. W. Mersh Strong.—*Rock Drawings from New Guinea.*

Wednesday, September 13.

19. Mr. A. Leslie Armstrong.—*The Maglemose Remains of Holderness and their Baltic Counterparts.*

The occurrence in the Holderness area of East Yorkshire of bone harpoons, and other relics of Maglemose culture, beneath deposits of lacustrine peat, occupying the sites of extinct meres, and associated with an Ancylus fauna, point to the presence there in late Palæolithic times of a people having cultural affinities with the raft-dwellers of Maglemose, in the island of Zeeland and Baltic sites of the Ancylus Lake. No evidence of Maglemose culture has hitherto been forthcoming in Britain, and the Holderness sites appear to represent its most westerly extension. The two harpoons recovered are of excellent workmanship, and preserve in design and detail the Magdalenian tradition more strikingly than is observable in any figured examples from Maglemose.

The Baltic counterparts were discussed; also the theories relative to the racial identity of the Maglemose people. Numerous pit dwellings, assigned by the late Canon Greenwell and by Sir W. Boyd Dawkins to a very early phase of Neolithic culture, have been located in proximity to the sites from which the Maglemose remains have been recovered. Is there any cultural connection between the pit dwellers and the Maglemose people?

20. Mr. W. Collingwood.—*Tenth Century Art in the Danelaw.*

The art of this period and district exists chiefly in stone monuments, of which there are fairly abundant remains.

The Danes who invaded Northern and Eastern England had settled down about A.D. 900, adopting much of what they found here. For their monuments they employed such Anglian carvers as survived, and for the first quarter of the century debased traditions of Anglian art were followed. With the establishment of the Viking kingdom of York in connection with Ireland some Celtic motives were brought in, and Danish taste gradually prevailed, creating in the second half of the century the Anglo-Danish style of ornament, seen especially in East Yorkshire.

From about 920 Norse from the Irish Sea coasts began to settle Cumberland and to penetrate Lancashire and parts of Yorkshire. They seem to have introduced the wheel-head form of cross, with design in their own style, modified by Irish art and superadded to the Anglian tradition already known in the districts they settled. From this combination of influences many interesting and picturesque works resulted, reaching a high standard in the Gosforth cross and others of the end of the tenth century.

21. Prof. A. Mawer.—*Place-Names and Ethnology in the East Riding of Yorkshire.*

The place-names of the East Riding are almost exclusively English or Scandinavian, and there is very little trace of any Celtic element. In that

English element we may note the numerous *-inghams* so characteristic of Eastern England, and the very small number of *-worths* linking it with Lincolnshire and Norfolk. In this latter it presents a strong contrast with the West Riding, which here shows Southern and Western affinities. This is true also of the *leys*, but the differences here may in part be due to physical causes. On the Scandinavian side the East Riding stands midway between the other two, but it is difficult to judge the West Riding as a whole. Many of the place-names and certain of their forms show clearly that some common Anglo-Scandinavian speech probably prevailed over the whole area, and right down to the thirteenth century we have evidence of the use of alternative English and Scandinavian forms of the same name. Great care is needed in applying certain of the common tests for the presence of Scandinavian settlers, and certain suffixes present special difficulties—*e.g. ing*, *ham*, and *thorp*. There is a greater proportion of the last-named in the East Riding than anywhere else, except in Lincolnshire and Notts and Leicestershire. Is this due to Danish or Anglian influence?

These and all problems of the origin and distribution of place-names need to be studied in the light of historical, archæological, and topographical facts, and with the aid of the comparative evidence for at least the whole of England.

SECTION I.—PHYSIOLOGY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 409.)

Thursday, September 7.

1. Dr. F. C. Eve.—*Life and Energy: an Interpretation.*

2. Dr. T. Ritchie Rodger.—*The Effect of Loud Noises on the Cochlea.*

The results of an investigation undertaken by the author into the deafness of boilermakers are summarised, and these are compared with the findings of German and other observers who have experimented with animals exposed to loud sounds.

The contention is that the changes indicated clinically and demonstrated post-mortem support the theory that the cochlea is adapted for the differentiation of sound.

3. Dr. J. E. Bannen.—*The Physiology of the Gastro-Intestinal Tract from the Radiological Aspect.*

4. Dr. G. Wilkinson.—*The Mechanism of the Cochlea, with especial reference to the Inertia of the Contained Fluids.* Illustrated by a Demonstration of a Working Model showing the Resonant Vibration of Immersed Strings.

5. Prof. A. V. Hill, F.R.S.—*Athletics and Oxygen Supply.*

Friday, September 8.

6. Presidential Address by Prof. E. P. Cathcart, F.R.S., on *The Efficiency of Man and the Factors which Influence it.* (See p. 164.)

7. Joint Discussion with Section M on *The Vitamins.*

(*a*) Opener: Prof. J. C. Drummond.

(*b*) Capt. John Golding, D.S.O.—*An Estimation of the Practical Agricultural Importance of Vitamin A in Feeding Pigs.*

(*c*) Dr. A. Seidell.—*Further Studies on the Isolation of the Antineuritic Vitamins.*

By means of a relatively simple process, a stable, semi-solid extract of highly active vitamin has been obtained from fresh brewer's yeast. This has been used for fractionation experiments with silver nitrate and ammoniacal silver nitrate as precipitating agents. The distribution of the active vitamin in the several fractions has been followed by means of feeding experiments on pigeons. The activity and general character of the several precipitates have been determined.

8. Prof. W. Storm van Leeuwen.—*Experimental Studies on Hypersensitiveness.*

The action of drugs on isolated organs in Tyrode Solution is an instance of hypersensitiveness, since the drugs are not *inhibited* by blood constituents.

Drug action can be *augmented* by substances which have no stimulating action *per se.*

These principles suffice to explain hypersensitiveness to drugs in man.

(*a*) Blood of cases of hypersensitiveness to aspirine shows diminished fixation of aspirine.

(*b*) Aspirine, uric acid, oleic acid, and similar substances augment drug action.

(*c*) Aspirine heightens sensitiveness of guinea-pigs to anaphylactic shock.

Conclusion :—Hypersensitiveness to drugs is due to (*a*) lack of fixing power of blood for the drug; (*b*) augmentor action in allergic reactions.

This explains why the symptoms of hypersensitiveness to different drugs show little differentiation.

9. Prof. W. Storm van Leeuwen.—*A Contribution to the Cause and Treatment of Bronchial Asthma, Hay Fever, and Allied Conditions.*

In asthma, hay-fever, and allied conditions an allergic disposition exists with following characteristics :—

(*a*) Skin reactions with *several* proteins; hypersensitiveness to *one* protein is exceptional (no hypersensitiveness of skin to various sera, casein, peptone, histamine; (*b*) 'crise colloidoclasique'; (*c*) blood extracts contain poison stimulating smooth musculature (relation with uric acid metabolism); (*d*) hypersensitiveness to tuberculin (relation between *a* and *d*).

Treatment—Specific treatment often impossible. The author's treatment :—(*a*) tuberculin in low doses; (*b*) regulation of diet. In addition to that, specific treatment peptonetherapy (Auld) vaccine, etc. Author's results: 52 per cent. cured, 38 per cent. improved, 10 per cent. failures.

10. Mr. J. Barcroft, F.R.S.—Lecture on *The Recent Expedition to the Andes for the Study of the Physiology of High Altitudes.*

Monday, September 11.

11. Joint Discussion with Section A on *Physical Instruments for Biological Purposes.* (See p. 353.)

12. Dr. F. W. Edridge-Green, C.B.E.—*Colour-Vision Theories in Relation to Colour-Blindness.*

Whilst many colour-vision theories explain equally well the facts of colour-mixing, none of the former theories will explain the facts of colour-blindness.

The facts of colour-blindness are totally opposed to any theory which assumed elementary sensations of which the other colour sensations are compounded. A theory of colour vision should explain how 50 per cent. of dangerously colour-blind can pass the wool test, the varying size of the monochromatic divisions in the spectrum with different degrees of colour perception, why the trichromic mark out about half the normal number of monochromatic divisions in the spectrum, designate yellow as red-green, and have an increased simultaneous colour contrast? When there are three definite colour sensations how can colour-blindness be explained? When a colour-blind person is tested by making him match a white with a mixture of red, green, and violet, he may put too much red in the mixture, and then too much green, and also agree with the normal match; he may only agree with the normal match when the comparison white light is increased in some cases and in others diminished in intensity. Cases of shortening of the red end of the spectrum without defective colour discrimination require to be explained. A man may also make an anomalous equation, putting too much green or too much red in the mixture, without any evidence of colour-blindness.

13. Dr. F. W. Edridge-Green, C.B.E.—*The Necessity for a Standard of White.*

On account of the varying physical character of white light, even of daylight, which varies at different times of the day, and still more with light from artificial sources, any record for physiological purposes should contain a reference to the source of white light, which should be used for purposes of comparison and which can be reproduced by another observer. Very misleading results are obtained by the omission of a comparison light. For instance, the table of complementary colours given by Helmholtz was made without any comparison white light; his table does not agree with that of other observers.

Tuesday, September 12.

14. Dr. P. M. Tolmie.—*The Cytology of the Blood and the Source, Development and Function of Some of the Corpuscles.*

15. Dr. J. H. Burn.—*The Physiology of Sweating.*

After denervation of the limb of a cat, in which either the whole nerve supply or the sympathetic fibres only are involved, changes taking place in the response of the sweat glands to pilocarpine are accompanied by parallel changes taking place in the dilatation of the limb in response to histamine injected intravenously. The suggestion arising is that the activity of the sweat glands, when stimulated by pilocarpine, depends on the local capillary tone, a tone independent of the arterial tone in the limb, and that the maintenance of this tone is dependent amongst other things on the integrity of the sensory nerves.

16. Prof. W. D. Halliburton, F.R.S.—Lecture on *Our Bones and Teeth.*

SECTION J.—PSYCHOLOGY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see pp. 409-10.)

Thursday, September 7.

1. Dr. C. S. Myers.—*Industrial Psychology and the Efficiency Engineer.*

2. Dr. G. H. Miles.—*Applications of Psychology to Breakage Problems in Industry.*

3. Mr. J. Seebohm Rowntree, Jr.—*Practical Applications of Vocational Tests.*

4. Mr. Eric Farmer.—*The Value of Output Curves as Measures of Fatigue.*

5. Dr. C. H. Northcott.—*Instincts and Society.*

This paper was a denial of the stress laid upon instincts as basic in the explanation of social action, and an endeavour to set out the factors involved in the psychology of society.

If the problem of sociology is held to be the explanation of collective behaviour, the doctrine of instincts affords grounds for postulating a considerable degree of uniformity of nature and response. But what other contribution can it make?

Treated genetically, collective behaviour is a product of inter-stimulation between beings alike in kind, to whom companionship, toleration, and co-operation are relatively easy and agreeable. Common danger or common opportunity calls forth similar behaviour, which, becoming the subject of agreement, leads to customs and collective decisions. These in their turn exercise social pressure and social control, regulating the exercise of the instinctive reactions of individuals.

A fuller psychological analysis robs instincts of their prestige as explanations of collective behaviour, and gives a larger place to customs, habits, institutions, and social organisation.

6. Miss May Collins.—*Experiments on Colour-Blindness.*

7. Dr. Ll. Wynn Jones.—*The Technique of Group Testing.*

Friday, September 8.

8. Mr. H. Binns.—*Industry and Education.*

9. **Joint Discussion** with Section L on *Psychoanalysis and the School.* (See p. 403.)

10. Mr. W. Whately-Smith.—*Entia præter Necessitatem. . . .*

11. Prof. T. H. Pear.—*The Acquisition of Skill in Reference to Training in Industry.*

12. Dr. P. Sargeant Florence and Dr. A. H. Ryan.—*Spoilt Work in Relation to Hours of Labour.*

Monday, September 11.

13. Dr. G. Auden and Mr. C. Burt.—Discussion on *Moral Imbecility.*

14. Dr. William Brown.—*Auto-Suggestion and the Will.*

15. Presidential Address by Dr. C. S. Myers, F.R.S., on *The Influence of the late W. H. R. Rivers on the Development of Psychology in Great Britain.* (See p. 179.)

16. Dr. C. W. Kimmins.—*The Sense of Visual Humour in Children.*

McDougall's new theory of laughter—A comparison of the results obtained by investigations of verbal and visual humour—What sights children laugh at most at different ages—Results of analyses of children's records—The effect of rapid physical growth on the sense of humour—The difference in the sense of visual humour in boys and girls under varying conditions at the same age—The important part played in visual humour by the feeling of superiority—The theories of laughter of Bergson, Freud, Boris Sidis, Sully, and McDougall.

Tuesday, September 12.

17. Joint Discussion with Section H on *Mental Characters and Race.* Opener: Prof. J. L. Myres.

18. Dr. F. C. Shrubsall.—*What is Mental Deficiency?*

19. Dr. G. A. Auden.—*Types of Mental Deficiency.*

SECTION K.—BOTANY.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 410.)

Thursday, September 7.

1. Dr. W. H. Pearsall and Prof. J. H. Priestley.—*Leaf Growth.*

Leaf growth is considered from the point of view of form variations in given types of leaves. Since the latter, in any species, appear to be largely due to external factors, a starting point is presented for the consideration of the internal mechanism affecting leaf form. Two main internal factors are recognised : (1) hydrostatic pressure, (2) the permeability of the cell walls in the meristematic tissues.

Leaf growth ceases, or is much restricted, by negative hydrostatic pressures, and may be increased by positive pressures. Daily and seasonal variations in growth-rate agree with this fact, also the variations in the size and form of leaves produced under experimental conditions.

Other modifications of leaf shape under diverse light conditions appear to be due to alterations in permeability of the walls of the meristematic cells.

The supply of nutrient solution to the growing points is affected not only by these two factors, but is also a resultant of the position of the growing points in relation to vascular supply.

2. Dr. I. Soar.—*The Structure of the Endodermis in Some Gymnosperm Leaves.*

The leaves of some Gymnosperms, and especially those of the Abietineæ, show a well-marked endodermis, the cell walls of which are usually thickened. Where modification of the endodermal wall occurs, the radial walls are suberised and pitted, whereas the transverse walls are unpitted. In both cases the suberin is deposited as a surface layer on either side of a lignocellulose core. The nature of the tangential walls varies; they are frequently lignified and

suberised with pits in the suberised membrane. In some cases these walls, especially the inner tangential ones, consist of cellulose.

The suberisation and lignification of the radial walls renders the endodermis relatively impervious to the passage of water through the walls alone. Thus the transpiration current must flow largely through the endodermal cell, and it is probable that the protoplasm exerts some control over the rate of flow.

3. Presidential Address by Prof. H. H. DIXON, F.R.S., on *The Transport of Organic Substances in Plants.* (See p. 193.)

4. Joint Discussion with Section B on *Photosynthesis.*

(*a*) Dr. F. F. BLACKMAN, F.R.S.—*The Biochemical Problems of Chloroplastic Photosynthesis.*

1. The active system of the living cell and its catalytic components. The chlorophyll pigment component: its quantity and surface: Willstätter's chlorophyll numbers. The protoplasmic component; its possible enzymatic nature. Cases of lack of co-ordination between the two components.

2. The chemical reactions of reduction and condensation. The spontaneous and catalysed metamorphosis of CO_2 to substances of high anabolic potential.

3. The energetics of these reactions: the problem of evaluation of energy absorbed by chlorophyll in relation to chemical and physical work done: comparative efficiency of incident energy of different wave-lengths.

4. Factors controlling the magnitude of photosynthesis; quantity of incident energy and of incident CO_2. Relation to accumulation of products and to toxic effects. The temperature coefficient of the reaction.

5. The case of the chemosynthetic micro-organisms carrying out reduction of CO_2, in absence of 'light'-energy, by the oxidation of nitrogen, sulphur, or hydrogen. The energy-balance of these processes.

(*b*) Professor E. C. C. BALY, C.B.E., F.R.S.—*Photosynthesis.*

In every chemical reaction it is necessary to supply energy to the molecules in order to cause them to react. In some cases this energy change is effected by the solvent, but in other cases the increment of energy required is far too large to be realised in this manner. This latter condition always obtains in highly endothermic reactions, and it is often necessary to supply the energy in the form of light, when the resulting process is called a photochemical one.

The most interesting of all reactions is the photochemical conversion of carbonic acid into formaldehyde, since this not only marks the first step in the formation of sugars, starches, and celluloses, but it also plays a fundamental rôle in the synthesis of the nitrogen products of plant life. This reaction, indeed, forms the true key industry of all life.

In order to enable this reaction directly to take place, considerable energy is required so as to bring the carbonic acid molecule into the reactive form, and this is secured when the molecule is exposed to light of very short wave-length. It has been found, however, that in the presence of a coloured substance of basic properties the reaction takes place in visible light. There is little doubt that in the plant the reaction takes place in three stages:

1. Chlorophyll A + H_2CO_3 + light = Chlorophyll B + formaldehyde.
2. Chlorophyll B + Carotin = Chlorophyll A + Xanthophyll.
3. Xanthophyll + light = Carotin + Oxygen.

The formaldehyde when freshly synthesised is endowed with an extraordinary reactivity, which is best expressed by the formula CHOH. In the presence of potassium nitrite, which is known to exist in the growing leaf, combination at once occurs to give formhydroxamic acid. This substance reacts with more of the formaldehyde to give on the one hand α-amino acids, and on the other, nitrogen bases, such as glyoxaline, pyridine, pyrrole, quinoline, and indole. These substances are also produced in highly reactive forms, so that combination at once ensues to give substituted amino acids, which then condense to give proteins. The nitrogen bases which do not condense with the amino acids undergo further condensation to give alkaloids.

The excess of the photosynthesised formaldehyde beyond that used in these reactions undergoes polymerisation to hexoses, and these in their turn give sucrose, starches, and celluloses. All the complex compounds are the natural and inevitable results of the photosynthesis of formaldehyde in the presence of small quantities of potassium nitrite.

(*c*) Mr. G. E. Briggs.—*The Efficiency of the Photosynthetic Mechanism of Green Plants for Different Wave-lengths of Incident Radiation.*

The desideratum is to know what fraction of the radiant energy absorbed by the photosynthetic pigment or pigments is utilised in the conversion of carbon dioxide into the primary product of photosynthesis, and the relation between this fraction and the wave-length of the incident radiation.

The problem resolves itself into deciding, firstly, which of the pigments plays a direct part in the photosynthetic process; secondly, what portion of the radiant energy absorbed by the photosynthetic organ is absorbed by the particular pigment or pigments; and, finally, what portion of the incident energy is used for the conversion of the carbon dioxide.

(*d*) Prof. I. M. Heilbron and Dr. C. Hollins.—*Some Speculations on the Phyto-synthesis of Plant Products.*

As hexose sugars have been proved to be the sole products of the photosynthesis of the nitrogen-free compounds in the green leaf, the formation of the innumerable other substances commonly met with in the plant must necessarily be produced at some later stage, which is probably closely connected with respiratory action.

An examination of the constitution of plant compounds immediately brings to light the fact that the predominant carbon nuclei in these are simple multiples of a C_5 unit. Thus the terpenes are $C_5 \times 2$, the sesquiterpenes $C_5 \times 3$, which multiple also includes the flavone, flavonol, and anthocyan pigments. Phytol, $C_{20}H_{39}OH$, the alcoholic constituent of the chlorophyll molecule, is $C_5 \times 4$, while the closely related pigments carotin, $C_{40}H_{56}$ and xanthophyll, $C_{40}H_{56}O_2$, are based on $C_5 \times 8$. It is suggested as a plausible hypothesis that the C_5 unit is formed by oxidation of ω-hydroxy methyl furfuraldehyde, which is derived from the primarily synthesised hexose by loss of water. If the possibility of the production of furane derivatives in this way be admitted, then the conversion of these into pyrroles by means of ammonia or methylamine could readily take place by perfectly straightforward reactions.

(*e*) Dr. F. C. Eve.—*Photosynthesis from the Energy Aspect.*

Friday, September 8.

5. Prof. J. McLean Thompson.—*The Meaning and Evolution of Some Floral Characters.*

The origin, meaning, and modification of systematic characters claim intensive study. The structural expressions of such characters demand developmental inquiry linked with exact knowledge of the chemistry of the cell.

In the present communication certain structural aspects of the origin and development of floral characters were discussed. The origin and meaning of the characters themselves are subjects of speculation. The aim in view is to point to the need of study of the changing cell-chemistry from the initiation of a systematic character to its establishment. It is held that by such study a knowledge of evolution may be materially advanced.

6. Mr. J. Walton.—*The Physiological Anatomy of the Fossil Genus Rhexoxylon compared with that of some modern Lianes.*

The genus *Rhexoxylon* (Bancroft) shows features in the distribution and histology of its xylem analogous to some which appear in modern lianes, in particular to certain members of the Malpighiaceæ. *Rhexoxylon* occurs in beds of the Stormberg series of South Africa, and the name *Antarcticoxylon* (Seward) of a fossil stem from the Beacon Sandstone, S. Victoria Land, must be regarded

Addendum, p. 396. The following should be added to the Discussion on Photosynthesis :

(*f*) Prof. M. C. POTTER.—*Photosynthesis: Electric Energy.*

In addition to the calories set free during the fermentation of sucrose and glucose by yeast, there is also a liberation of E.M.F. of approximately .3 volts to .5 volts.[1] Thus the equations should express this E.M.F. as well as the difference of calories :

$$\begin{array}{lcl} C_{12}H_{22}O_{11} + H_2O & = & 4C_2H_5OH + 4CO_2. \\ 1352.7 \text{ cal.} & & 4\ (325.7) \text{ cal.} \end{array}$$

The equation should therefore read :

$$C_{12}H_{22}O_{11} + H_2O = 4C_2H_5OH + 4CO_2 + 49.9 \text{ cal.} + x \text{ (E.M.F.)},$$

where x is a constant at present undetermined, but .3 volts are obtained during the fermentation.

A similar phenomenon necessarily enters into enzyme action :

$$\begin{array}{lcl} \text{Sucrose} & & \text{Dextrose} \quad \text{Laevulose} \\ C_{12}H_{22}O_{11} + H_2O & = & C_6H_{12}O_6 + C_6H_{12}O_6. \\ 1352.7 \text{ cal.} & & 673.7 \text{ cal.} \quad 675.9 \text{ cal.} \end{array}$$

During this reaction .03 volts are obtained, and the equation should be expressed :

$$C_{12}H_{22}O_{11} + H_2O = C_6H_{12}O_6 + C_6H_{12}O_6 + 3.1 \text{ cal.} + x \text{ (E.M.F.)}.$$

The amount of electricity has not at present been determined, but it would appear to bear some definite relation to the calories.

It may be suggested that as the atom is built up of electrons and protons, so must these also form part of the structure of the molecule. This E.M.F. therefore expresses the difference between the electrons and protons in the various molecules, just as the calories express the difference between their heats of combustion.

It may be noted that the action of the enzyme is from a higher potential to a lower one, and that during the reaction energy is liberated. For an enzyme to act synthetically energy must be supplied for the work performed, and hence it is improbable that synthetic enzymes exist.

The view is put forward that electrons and protons form an integral part of the carbohydrate, and are required during the synthesis of these bodies (and all intermediate ones), and that they may be found to be a limiting factor in photosynthesis ; and further, that there may be endo-electric and ex-electric reactions analogous to the endothermic and exothermic reactions.

[1] *Proc. Roy. Soc.*, B, vol. 84, 1911.

as a synonym. This affords evidence for supposing that the Beacon Sandstone is of an Upper Triassic or Rhætic horizon.

7. Dr. H. S. Holden and Miss Dorothy Bexon.—*On the Seedling Structure of Acer pseudoplatanus.*

The embryo of the sycamore, at the time the fruits are shed, consists entirely of parenchyma, the vascular structures being represented by desmogen strands. It contains abundant food reserves, consisting of : (*a*) starch, which is uniformly distributed through the tissues, (*b*) proteins, of which there are at least two, existing in colloidal solution.

In spring the protoxylem and first metaxylem elements are differentiated prior to germination. Each cotyledon is traversed by a midrib, with a stout lateral and a smaller marginal bundle on either side. At the base of the cotyledon the metaxylem of the midrib separates into lateral groups, which unite with the converging laterals and marginals, so that four massive bundles in the diagonal position enter the hypocotyl with isolated protoxylems in the cotyledonary plane. These ultimately give rise to a tetrarch root.

The histology of the xylem in the hypocotyl and young epicotyl shows that the component elements exhibit a relatively wide variation in type of pitting and other characters which may provide phyletic data.

8. Miss T. L. Prankerd and Miss F. M. O. Waight.—*On the Presentation Time and Latent Time for Reaction to Gravity in Pteridophytes.*

The object of the present work is to express quantitatively some of the facts of irritability already described for fern fronds (*P.R.S.*, B., 93, 1922). An attempt has already been made to measure the intensity of gravitational irritability in the fronds of *Asplenium bulbiferum* by finding the presentation time, which is shown to vary greatly with the age of the frond, the temperature, and possibly with some other factors. At 20° C. the range for presentation time is at least 8 hours$-\frac{1}{2}$ hour, and for the corresponding latent period 16$-$4 hours, the latter thus bearing an increasing ratio to the former.

Effect of temperature on presentation and latent time. Connection with growth and nutation. Comparison of results with those obtained for other plants.

9. Dr. W. L. Balls.—*The Growth Structure of a Cell-Wall.*

Published observations on the structure of the epidermal seed hairs of the cotton plant have shown that the secondary cellulose of the wall may consist of a number of concentric growth-rings, deposited centripetally, one each day; further, that through these growth-rings runs a radial structure, or even two such structures, visible to some extent in the natural state, but more clearly defined after appropriate treatment. Thus, each growth-ring consists of a number of spiral fibrils, the direction of the spirals frequently reversing. The primary spiral pattern for any one hair appears to be laid down in the primary wall during growth in length, and thereby to pre-determine the same pattern for all the succeeding growth-rings of the secondary wall.

Incidental observations suggest that this kind of structure is not peculiar to cotton. Various points of interest arise with respect to the composition of cellulose, the mechanism of growth, inhibiting factors, the development of form, and a possible geometric structure in the cell. Generally, the observations suggest that it may be practicable to attack growth problems from the sub-microscopic side.

10. Prof. A. H. Reginald Buller.—*The Organisation of the Hymenium of the Common Mushroom and its Allies for the Production and Liberation of Spores.*

The author, using a new method of investigation, has succeeded in elucidating the time and space relations of the elements making up the hymenium in the common mushroom and its allies.

The gills of *Psalliota campestris* are finely mottled, and the mottling is of the same nature as that of *Panæolus campanulatus.* The basidia of the darker

hymenial patches bear pigmented spores, while those of the lighter patches do not.

The hymenium of *Psalliota campestris* consists of basidia and paraphyses. The paraphyses are permanently sterile elements. The basidia on any one small portion of the hymenium can be classified as past-generation basidia, present-generation basidia, coming-generation basidia, and future-generation basidia.

11. Dr. MALCOLM WILSON.—*The Cytology and Life-history of Tuburcinia.*

The two British species of *Tuburcinia* show considerable differences.

In *T. primulicola* the mycelium, which is perennial in the rootstock, penetrates the entire plant, and produces conidia on the stamens. These conidia may be distributed, along with the pollen, by insects. They are unicellular and often conjugate in pairs, the united conidia then giving rise to a secondary conidium which may grow out into a germ-tube. Chlamydospores are found between the ovules in flowers which previously produced conidia. The chlamydospore masses germinate as soon as free, each spore producing a germ-tube which bears a whorl of 3-5 primary sporidia. These occasionally conjugate in pairs, but generally each produces a secondary sporidium without conjugation.

In *T. Trientalis* certain infected rhizomes produce shoots which bear stalked conidia on the under surface of the leaves. These conidia may form secondary conidia without conjugation.

The plants bearing conidia also produce chlamydospores in the cortex and pith of the stem. These germinate in the late autumn, each spore producing a germ-tube which bears a whorl of 8 primary sporidia. Conjugation takes place regularly between the primary sporidia; secondary sporidia are produced, and these on germination may bring about the infection of the rhizomes.

In *T. primulicola* the cells of the perennial mycelium are generally uninucleate. The conidia are uninucleate, and during conjugation a nucleus passes from one conidium into the other. The secondary conidia are binucleate. These presumably give rise to the mycelia consisting of binucleate cells. There is a nuclear fusion in the young chlamydospore, the mature spore being uninucleate. The primary sporidia are generally binucleate, occasionally uninucleate. The secondary sporidia are sometimes binucleate, sometimes uninucleate.

In *T. Trientalis* the mycelium present in the host plant consists of binucleate cells. It has been impossible yet to determine the nuclear condition of the chlamydospores and sporidia. It may be concluded that in *T. primulicola* the conjugate nuclear condition is brought about during the conjugation of the conidia. In *T. Trientalis* it is suggested that the conjugate condition is produced at the fusion of the sporidia.

12. Miss K. B. BLACKBURN and Dr. H. HARRISON.—*The Meiotic Phase in the Salicaceæ.*

The cytological behaviour in the *Salicaceæ* is not in any way comparable with that of the genus *Rosa*; the majority of the species of both genera show perfect reduction divisions. When, however, this fails, as, for example, in *Salix fragilis*, the phenomena observed resemble the conditions seen in the hybrid *Oporabiæ*.

The fundamental chromosome number in both *Salix* and *Populus* is 19, although many species of *Salix* are tetraploid, and at least one is hexaploid.

Somewhat unexpectedly, in the homogeneous *Capreæ* group the species do not agree as to the haploid number of chromosomes, *S. aurita* and *S. cinerea* being characterised by 38 and *S. Caprea*, in the main, by 19.

Compound chromosomes occur both in *Salix* and *Populus*, and some evidence exists of the presence of an unequal chromosome pair in the male plants.

A metamorphosans variety of *S. Caprea* was examined, and displayed the number of chromosomes proper to the species.

13. Mr. W. C. F. NEWTON.—*Somatic Chromosomes.*

The somatic divisions have been studied in several Spermatophyta, and the pairing of the allelomorphs and the longitudinal fission of the chromosomes differentiated.

Monday, September 11.

14. Joint Discussion with Section D on *The Present Position of Darwinism.*

(*a*) Dr. J. C. Willis, F.R.S.—*The Inadequacy of the Theory of Natural Selection as an Explanation of the Facts of Geographical Distribution and Evolution.*

Darwin's immortal service to science consists in the permanent establishment of the doctrine of evolution. This he effected by devising the simple and beautiful mechanism of natural selection of infinitesimal variations—the principle usually known under the name of Darwinism.

This doctrine makes several assumptions : among others, that variation resulting in evolution is (1) continuous, (2) hereditary, (3) differentiating, (4) selected, and (5) that the necessary variations appear. For all of these the proof is as yet insufficient.

Work carried on during the last thirty years has finally led to the demonstration that the phenomena both of evolution and of geographical distribution may be represented by hollow curves, which are always of the same type, which are closely parallel in both plants and animals, and which may be found in thousands of cases. Taken in averages of tens of allied forms, area occupied goes with age of species and with size of genera. In other words, size and area show the same phenomena, both increasing with age. Natural selection, a differentiating cause, could not produce such uniformity of expression. Evolution would appear to have unfolded itself with time upon a dominant plan, natural selection simply acting as an agent destroying the unfavourable (and probably many other) variations.

(*b*) Mr. G. Udny Yule, F.R.S.—*A Mathematical Conception of Evolution based on the Theory of Age, Size, and Space.*

If, comparing allied groups, the more widespread species are on the whole the older, and the larger genera on the whole the most widespread, it follows that the size of the genus must on the whole be an index of its age. This suggests that species may be regarded as thrown by the genus much as offspring are thrown by a stock, and that the number of species originating from a given initial species will increase in geometric ratio with the time.

The forms of frequency distribution for numbers of genera with given numbers of species to which this conception leads are shown to be in accordance with the facts, and the possibility is suggested of determining from such distributions the ratio between the rates of increase of genera and species and the age of the family in terms of the doubling period for species.

(*c*) Mr. C. Tate Regan, F.R.S.

(*d*) Prof. W. Johannsen.

(*e*) Mr. J. T. Cunningham.—*Origin of Species and Origin of Adaptations.*

Darwin's theory of Natural Selection is based on the assumption that the species and its diagnostic characters, either visible or invisible, are adapted to some special habits or conditions peculiar to each species; in other words that the characters, or the successive steps in the evolution of characters, have been selected. Evidence in the vast majority of cases is against this assumption and in favour of the conclusion that diagnostic characters have nothing to do with adaptation. It is now known that many species spontaneously give off mutations, and that those are not due to influence of conditions in experiment or cultivation. They occur in Nature, and some of them may be incapable of survival in Nature, but this does not imply that those which can survive are due to selection. So far I am in agreement with Mr. Udny Yule and Dr. Willis. But I do not understand what is meant by the statement that the area occupied goes with age of species. How are we to know which species is the older? It may be legitimate to infer age from area occupied, but not to assume age except from morphological evidence. Man is cosmopolitan in distribution and is one of the

newest species (or genera). All these questions have nothing to do with adaptation. Adaptation is not the same as utility. There may be characters which are useful in reference to certain conditions of life whose origin was independent of those conditions. But it is more important to consider cases in which structures have been changed to perform new and essential functions in relation to changed conditions, *e.g.* respiratory organs in embryos, larvæ and adults, or mammary glands. In most cases these organs show recapitulation which is not (with a few special exceptions) exhibited by mutations. The modern discoveries concerning hormones or internal secretions show how such modifications with recapitulation may have been produced by stimuli and functional exercise.

(*f*) Dr. H. Wager, F.R.S.

15. Prof. J. H. Priestley.—*The Endodermis: A Study in Causal Anatomy.*

In the angiosperm root primary, secondary and tertiary stages succeed one another during the development of the endodermis. The development of the primary endodermis behind the root growing-point appears to be causally connected with (1) the fat metabolism of the apical meristem, (2) the changes proceeding in the membranes of the meristematic cells, (3) the diffusion of substances from the differentiating phloem. The appearance of the secondary endodermis in the root is to a certain extent under experimental control.

In the angiosperm stem a primary endodermis is usually present in underground rhizomes, submerged water-plants, and in etiolated shoots, but it may be replaced by a starch sheath. The relation of the starch sheath to the primary endodermis is considered.

The appearance of a secondary endodermis in the stem is not necessarily preceded by any primary stage. The conditions under which the secondary endodermis appears in the stem are reviewed with special reference to *Rubus Idæus*, L., and *Camellia japonica*, L.

16. Dr. W. Robinson and Mr. H. Walkden.—*Critical Observations on Crown Gall in Chrysanthemum frutescens.*

The work of Erwin Smith and others regarding *Bacterium tumefaciens* as the cause of crown gall is confirmed. Smith assumes, but does not claim to have directly demonstrated, that the bacteria are present in the tumour cells. From the present work it is concluded that *B. tumefaciens* is always present in large numbers on the external surface, and sometimes on internal surfaces of galls. We have further failed entirely to demonstrate the bacteria within the tumour cells. All the effects are consistent with the action of increasing numbers of *B. tumefaciens*, at first from the wounded surface, later from the gall surface, from interstices of this or from internal surfaces.

Smith's work on secondary tumours and tumour strands has been critically repeated, galls similar in all respects and similarly distributed to the primary and secondary galls figured by Smith having been obtained. Most, if not all, of the secondary galls and tumour strands can be explained by the expansion during rapid growth of meristematic tissues in the vicinity of the inoculated wound rather than by the intrusive growth of tumour tissue in Smith's sense. The similarities in this respect between crown gall and malignant tumours are more apparent than real.

17. Prof. R. Ruggles Gates.—*Size-Inheritance in Plants and Animals.*

It has been customary to interpret size-inheritance in terms of several cumulative Mendelian factors. This method has been loosely used. Increased variability in the F_2 compared with the F_1 has been regarded as sufficient evidence for the view of multiple size-factors in inheritance. It has recently been shown, however, that in animal hybrids certain non-inherited characters exhibit greater variability in F_2 than in F_1. It may also be necessary to distinguish between general size-inheritance and the size of repeated parts, such as flowers on a plant.

In crosses between *Œnothera rubricalyx* with large flowers, and *O. biennis* (small flowers), the hybrids have been studied for five generations. The F_1

was uniform and intermediate. In later generations erratic segregation was obtained, with different sizes of flowers on the same plant, and frequently different lengths of petal in the same flower. Since the behaviour is not Mendelian it may be due to the distribution of cytoplasmic differences.

18. Professor Dame Helen Gwynne-Vaughan, D.B.E.—Popular Lecture on *Moulds*.

Tuesday, September 12.

19. Rt. Hon. Lord Lovat, K.T.—*The Position of British Forestry To-day.*

(i) Lack of a forestry conscience in Great Britain. Reasons why there is no well-informed public opinion on the subject of forestry :—

(*a*) Absence of State forests.

(*b*) Absence of communal forests.

(*c*) Forestry no part of the life of the nation.

(*d*) Forestry in the past a private hobby of rich men and enthusiasts.

(*e*) Absence of authoritative data on such subjects as yield tables, commercial returns, costing, &c.

Comparison with other European countries in these respects.

(ii) State forestry—its advantages and disadvantages. The importance of the rôle played by private forestry. Its strong and weak points.

(iii) Difficulties which beset the path of progress :—

(*a*) In private afforestation :—

1. Initial cost of establishing plantations.
2. Taxation.
3. Transport.
4. Legislation.
5. Markets.

(*b*) In State forestry :—

1. The importance of a settled State forestry policy.
2. The review of the departure from the policy laid down by the Acland Committee in 1917.
3. A summary of difficulties inherent in any new State enterprise.

(iv) Why ordered progress must be uncertain until the nation has a better grasp of forestry values, based on more exact knowledge :—

(*a*) Of the requirements of the nation for soft woods in peace and in war.

(*b*) Of the position of supplies of soft woods at home and abroad.

(v) An outline of possible future development.

20. Dr. A. W. Borthwick.—*Farm Forestry.*

The object of this paper was to call attention to the neglect in this country to turn to greater advantages, both direct and indirect, what in the aggregate amounts to a large area of land regarded as waste places, scattered about on farms, even in the most arable districts, such as banks, knolls, precipitous declivities, hollows, marshy places, &c. Trees will grow in such places, and produce fencing material and many other timber requirements of the farm. They will grow where no other crops will grow. They encourage the growth of other crops on adjacent lands by the shelter they afford. They effect an ameliorating influence on the local climate, and stock of all kinds thrive better on farms with properly located plantations in the form of shelter belts, clumps, and roundels. Attention was also called to the injudicious practice of overcrowding hedgerows and roadsides with timber trees as well as the toleration of single trees in arable fields.

21. Professor A. Henry.—*The Cultivation of Poplars.*

The history of the various species and hybrids of poplars suitable for cultivation on a commercial scale. Their silvicultural peculiarities.

In the afternoon Skipwith and Riccal Commons were visited.

Wednesday, September 13.

22. Dr. W. R. G. Atkins.—*Some Physical and Chemical Factors which affect Plant Distribution.*

In addition to temperature, moisture content, and intensity of illumination, the salt and hydrogen-ion concentrations of the situation appear to be important in determining distribution. This becomes clear when observations are made on the soil supporting the same species in different localities. The soil reaction may have a direct influence on the protoplasm of the roots, or it may act indirectly by altering the flocculation, and consequently the aëration, of the soil. It also affects the solubility of phosphates, iron salts, &c., causing certain plants to become chlorotic so that they are crowded out under natural conditions. Titrations of chloride, measurements of density, electrical conductivity, freezing-point, and possibly of the critical solution-point with phenol, of natural waters and soil extracts, may serve to map out the salinity of various plant habitats.

23. Prof. J. H. Priestley and Dr. J. Ewing.—*Etiolation.*

Plants show different types of growth reaction when grown continuously in darkness. Attention is here restricted to the reaction exemplified by *Vicia Faba*, L., *Pisum sativum*, L., &c. The structural modifications produced in these plants are consistent with the assumption that the meristem membranes, when the stem is growing in darkness, remain relatively impermeable to the supply of nutrient sap diffusing from the vascular bundles. The relative impermeability of these membranes is also shown to be in accordance with experimental and micro-chemical data.

The presence of a functional primary endodermis in the stems of these etiolated plants in regions where it is missing from the normal stem grown in the light is recorded, and the significance of this emphasised in relation to other structural features characteristic of etiolation.

In the upper region of these etiolated stems the primary endodermis is replaced by a starch sheath, and at the same time the rounded outline of the stem in cross-section changes, and it becomes angular, as in the stem grown in the light.

24. Miss Margery Knight.—*Nuclear Changes in Relation to Different Methods of Reproduction in the Ectocarpaceæ.*

The *Ectocarpaceæ* show great variation in the origin and development of motile reproductive cells. At different times and under different conditions one individual plant produces sexual, asexual, and neutral swarm-spores. These bodies show great variety in constitution, size, and behaviour. An inquiry into the nuclear structures accompanying these variations shows that in the life-cycle there is no rigidity of nuclear mechanism. The results of investigation demonstrate the extent to which cytological changes may be influenced by certain environmental factors.

25. Prof. J. C. Schoute.—*The Foliar Origin of the Internal Stelar Structure of the Marattiaceæ.*

26. Miss Violet M. Grubb.—*Notes on the Reproduction of certain members of the Rhodophyceæ.*

(*a*) *Porphyra vulgaris*, Ag.

Material gathered so far offers no decisive evidence of the presence of asexual spores in this species, as stated by Berthold, but the disputed fact of sexual reproduction is confirmed.

An attempt has been made to determine the fate of the carpospores by germinating them in cultures in order to verify the recent statement by Yendo on the production of micro- and macro-gametes.

(*b*) *Rhodymenia palmata*, Grev.

The only form of reproduction hitherto known has been by tetraspores, and as recently as 1919 Church quoted this species as one in which the sexual phase had been omitted from the life-cycle.

The carposporic frond has now been found both in the procarpial stage and with ripe carpospores, and can be distinguished from the tetrasporic frond. The procarps are developed in succession, and each has a long trichogyne. Carpospores are produced in pear-shaped cystocarps with ostioles, similar to those in *Rhodymenia palmata*, Grev.

Although the antheridial fronds have not yet been found, spermatia have been seen attached to the trichogynes.

EXHIBITS DURING THE MEETING: BOTANICAL SPECIMENS.

Botanical specimens were exhibited in a room near the Section Room, and others were shown by members of the Yorkshire Naturalists' Union in the Education Offices, Albion Street.

SECTION L.—EDUCATION.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 410.)

Thursday, September 7.

1. Presidential Address by Sir RICHARD GREGORY, on *Educational and School Science* (see p. 204), followed by a discussion.

2. Mr. R. C. MOORE.—*Advanced Instruction in Elementary Schools.*

For a number of years various voluntary attempts have been made to provide, to some extent, a rather more advanced education for the older or brighter children in the elementary schools. The Education Act, 1918, however, made it the duty of Local Education Authorities to provide such instruction.

Before advanced instruction can be given it is necessary that all the children should have a sound preparatory education. When the children have received this preparatory education, then advanced instruction of various kinds should be given to suit the abilities and needs of the pupils.

The advanced instruction should be given, as far as possible, under conditions which allow of a proper grading of the pupils. The teachers should be highly qualified, and suitable equipment and provision for practical work should be supplied.

The advanced instruction can be given at central schools or classes, or at the schools at which the pupils are already in attendance, or by the partial use of each of these methods. Local conditions will have to be taken into account in deciding which method is to be adopted.

It will be necessary to co-ordinate the advanced instruction in the elementary schools with the work of the secondary and technical schools, so as to allow of the passage from one type of school to the other of those pupils who have the necessary ability and attainments and who desire to obtain higher education.

Friday, September 8.

3. *Report of the Committee upon Training in Citizenship.* (See p. 337.)

4. *Report of the Committee upon The Teaching of Geography.*

5. Joint Discussion with Section J upon *Psychoanalysis and the School.*

(*a*) Dr. C. W. KIMMINS.

The need of assistance in dealing with the problem of the child who does not respond to normal methods of instruction—Children's dreams—The introvert and the extravert—The freedom of the child: the Montessori influence—The danger of the suppression of strong instinctive tendencies in methods of instruction—The necessity of the psychoanalysis of the teacher—The cultivation of self-discipline in the child—Day dreaming and the flight from reality.

(*b*) Dr. Crichton Miller.

The term 'psycho-analysis' has been claimed as referring exclusively to the theory and technique laid down by Professor Freud, and it is therefore necessary to use a different term, if we wish to express a view that does not accept all Freud's conclusions or coincide with his philosophy, however much it may owe to his original discovery. The term 'analytical psychology' will be used in this paper.

Our discussion raises first the question of the relation between teacher and doctor. Analytical psychology appeared first as a method of treating nervous disorders; this, however, is not its final function. Its real scope and value should be preventative; its application as universal as the accepted principles of hygiene; and its propaganda carried on by all who have a stake in the next generation. Hence its importance to teachers; and hence the necessity for teachers to understand and value it in their own experience.

The advent of analytical psychology marks a new era in education because it makes a new demand: that the teacher should know, not only his subject and his pupil, but *himself*. It follows that the chief function of analytical psychology in education is not to enable the teacher to analyse his pupils—a technical task for which he cannot usually have either the time or the training; nor is it to provide a new set of pigeon-holes into which he may thrust his own and other people's problems. Nor can analytical psychology supply purely temperamental defects; but it can help the teacher to recognise and to remedy failures of character-development in himself: the inherent childishness, the prejudice and self-deception which are the chief obstacles to understanding children and handling them wisely. An attempt is being made at the Tavistock Clinic in London to overcome the practical difficulty of making it possible for the teacher to acquire knowledge of analytical psychology.

Analytical psychology has a vital contribution to make to the problem of discipline. It reveals the failure of the two different methods represented by the training ship and the ultra-modern school: the problem cannot be solved by over-emphasising or by ignoring the demands of the herd on the individual, but only by interpreting them as wisely and patiently as possible.

If there are still teachers who maintain that analytical psychology is irrelevant to their work, they must be reminded that their failures will come to be judged by analysts later, who have to attempt the re-education of the adult who might have developed into a man, and instead developed into a neurotic.

(*c*) Dr. E. A. Hamilton-Pearson.

(*d*) Dr. R. G. Gordon.—*The Difficult and Delinquent Child.*

The importance of the question—Causes of delinquency—Feeble-mindedness—The psychopathic child—Pathological stealing, lying and truancy—The will to power—Aggressiveness and display—Effects of neglect—Neuroses, drugs, crime—Means of detection—Psychoanalysis, mental exploration.

Importance to the individual: Possibilities of cure.

Tests and routine examination: (1) Physical; (2) intelligence; (3) mental reactions.

Importance to the State: The Ohio Bureau of Juvenile Research.

The advantage of investigation—The necessity of qualification to conduct such investigation—The function of such research clinics—Advisory and executive.

(*e*) Prof. T. H. Pear.

6. Address upon *Imperial Citizenship* by the Rt. Hon. Lord Meston. (See p. 423.)

Monday, September 11.

7. Mr. Iveson S. Macadam.—*International Students' Organisations.*

8. Discussion upon *English as the Basis of National Education.*

(*a*) Mr. G. N. Pocock.—*The Teaching of English in Public Schools.*

The Report shows that schoolmasters have long been experimenting in the teaching of English, in isolation from one another. The problems they have set themselves to find out is whether English can become the natural and sufficient basis of all education in England. If this is so—and many of us are convinced that it is—it now remains to pool resources and enthusiasm, though without producing a stereotyped scheme.

It is essential to break down barriers between schools, and bulkheads between subjects in each school. If English is to be the basis of all education it must be taught scientifically as well as artistically. The basis of the scientific treatment is not grammar and analysis, but accurate observation and exact thought. The basis of the artistic treatment is self-expression, which can be trained through English Literature, by original composition both oral and written, and above all through the drama.

The object of this paper is to show how all this can be done—and is being done.

(*b*) Prof. Edith Morley.—*Consideration of the Report of the Departmental Committee.*

The general excellence of this Report is marred by its comparatively unsatisfactory treatment of the problem as it affects University 'Honour Schools of English.' The Committee have tried to hold the balance between the two branches of the subject, literature and language, but the compromises they propose are unlikely to meet the views of any experienced University teachers. English literature cannot be studied as it should be by Honours candidates, who read it only 'from Chaucer onwards,' for the English outlook on life has remained the same from the beginning, and neither Chaucer nor his successors can be properly understood without first-hand acquaintance with this permanent English point of view as exhibited in early writings. Nor can English prose style be adequately examined by those who are incapable of realising for themselves its continuity.

Similarly there is no break in the history of the language which is the medium of English literature. Consequently the recommendation to investigate the difficult problems of fifteenth-century English, or of later idiom, syntax and phonology, cannot be followed by those who are unacquainted with the earlier stages of the language. Still less can they profitably study place-names and family-names, which study, we are told, 'should form part of a living linguistic course.'

It is not possible for Honour students to begin the study of either language or literature with Chaucer, if they are to pursue their investigations by genuine University methods.

The Committee's arguments that the English Honour School should, for undergraduate students, consist of the two branches of literature and of language, are convincing. They are right, too, in the assumption that in the past too much Germanic philology has been demanded from those whose bent is primarily towards literature, and that this has often unduly curtailed the time available for reading. Changes in the right direction are already being made in most University curricula. Notably there are the alternatives permitted under the new Oxford scheme, or in the proposals for the new School of English at Liverpool, which is to supersede the present separate Schools of English Language and English Literature in that University. In some such way, and by means of some such evolutionary developments of the hitherto prevailing system, the just balance will eventually be struck. Progress is not likely to be achieved by means of the revolutionary proposals made by the Committee, which ignore the experience gained from the experiments of the past twenty-five years.

(*c*) Mr. J. H. Fowler.

Tuesday, September 12.

9. Consideration of the movement towards Individual Work in Schools, with special reference to experiments in Hull.

(*a*) Miss F. Sayer.—*Group Work in Infants' Schools.*

(*b*) Miss C. T. Cumberbirch.—*The Dalton Plan.*

1. The recent tendency towards group and individual work in upper classes in Elementary and Secondary Schools and in Training Colleges (leading on from Miss Sayer's paper)—seen in many directions and with varying aim—from silent reading to simple research.

2. Its concentration and development in the Self-Teaching Dalton Plan. A terse survey of the Plan, giving its broad aims.

3. The Plan—or modification—in local use. The work in four or five schools and colleges, under conditions varying as to: Specialist teachers; specialist rooms; allocation of free time; nature of assignment, of graph, and of test used. These points to be summarised.

4. Summary of the opinions of the head teachers and teachers on the development of the children and students in different subjects.

5. Discussion raised on several debatable points: The effect of the Plan on *esprit de corps*; the place and value in all teaching of: the class lesson, the teacher's personality; the difficulties met with in: (i) equipment, (ii) varying standards of work in children; (iii) the transition from Montessori to Dalton schemes.

6. Permanent value of the Plan ethically and intellectually.

10. Joint Discussion with Section G on *The Effect of Reformed Methods in Teaching Mathematics.*

(*a*) Prof. T. P. Nunn.—*The Principles of Formal Geometry.*

The question whether there should be a return to a standard sequence of theorems in elementary geometry is of less importance than the question whether the traditional basis of formal geometry should be retained. Euclid's system (apart from the preliminary doctrine of points, lines, and planes) rests upon two assumptions about the nature of space: (i) that it admits of congruent figures; (ii) that it permits one and only one line to be drawn through a given point parallel to a given line. There are strong reasons for adopting, instead of the latter, the assumption (ii) that space admits of similar figures, leaving the properties of parallel lines to be deduced therefrom instead of deducing the existence and properties of similar figures from the postulate of parallel lines.

(*b*) Mr. R. C. Fawdry.—*The Practical Result of the Reform.*

The reforms in mathematical teaching originated in a revolt against Euclid, followed by the demand from Technical Colleges that the mathematical equipment of the ever-increasing body of students should be wider in range and more practical in character.

The result was a reconsideration of the purport of mathematics as part of a general education. The slow progress of the majority was due to a course framed in the interests of specialists. The postponement of the more academic portions has allowed the early introduction of trigonometry and mechanics, and has enabled many to acquire a working knowledge of the calculus before leaving school.

Less emphasis is now laid upon bookwork; the examples are of a more interesting type and practical work has been introduced. The adoption of the newer methods is still far from universal. The older teachers and some Universities are still conservative, but progress is helped by the attitude of the Board of Education, the Civil Service Commissioners, and the holiday courses for teachers.

(*c*) Prof. M. J. M. Hill.—*Euclid's Exposition of the Theory of Proportion in the Vth Book.*

Euclid's Vth book has fallen into disuse not only in schools but also in Universities: this is probably due to the inversion of the natural order of ideas, to the omission of all explanation of the manner in which the fifth and seventh definitions were originally obtained, and to the unnecessary use of the properties of *unequal* ratios to prove properties of *equal* ratios. Although the book seems to be unsuited for school use, it is probable that if the idea of proportion be introduced from first principles the contents of the book would be of great value to University students commencing the serious study of the calculus.

SECTION M.—AGRICULTURE.

(For references to the publication elsewhere of communications entered in the following list of transactions, see p. 410.)

Thursday, September 7.

1. Mr. H. V. Taylor.—*Commercial Horticulture and Industry.*

2. Dr. A. G. Ruston.—*Yorkshire Farming as seen through Farm Costs.*

3. Prof. B. T. P. Barker.—*The Behaviour of Fruit Trees in Relation to Internal Nutrient Conditions.*

4. **Joint Meeting** with Sections A and F on *Weather Cycles in Relation to Agriculture and Industrial Fluctuations.* Opener: Sir W. Beveridge, K.C.B.

5. Mr. C. T. Gimingham.—*Some Notes on the Colorimetric Determination of Hydrogen Ion Concentration in Soils.*

6. Miss M. S. G. Breeze.—*Degeneration in Anthers of Potato.*

Friday, September 8.

7. Mr. E. A. Fisher.—*The Evaporation of Water from Soil.*

8. Prof. T. B. Wood, F.R.S., and Dr. J. W. Capstick, O.B.E.—*Influence of Temperature on Basal Metabolism.*

9. **Joint Meeting** with Section I on *The Vitamins.* (See p. 390.)

Saturday, September 9.

Farms in the Yorkshire Wolds were visited.

Monday, September 11.

10. **Presidential Address** by the Rt. Hon. Lord Bledisloe, K.B.E., on *The Proper Position of the Landowner in Relation to the Agricultural Industry.* (See p. 219.)

11. Sir Daniel Hall, K.C.B., F.R.S.—*Land Reclamation on the East Coast.*

12. Mr. J. A. Hanley.—*The Use of Lime in the North of England.*

13. Mr. N. M. Comber.—*The Limitations of Laboratory Methods of Lime Requirement Determination.*

In the afternoon Broomfleet Island, Blacktoft, was visited in order to see Natural and Artificial Warp Land.

Tuesday, September 12.

14. **Joint Discussion** with Section F on *The Possibility of Increasing the Food Supply of the Nation.* Opener: Sir John Russell, F.R.S.

REFERENCES TO PUBLICATION OF COMMUNICATIONS TO THE SECTIONS

AND OTHER REFERENCES SUPPLIED BY AUTHORS.

Under each Section, the index-numbers correspond with those of the papers in the sectional programmes (pp. 351–407).

References indicated by 'cf.' are to appropriate works quoted by the authors of papers, not to the papers themselves.

General reference may be made to the issues of *Nature* (weekly) during and subsequent to the meeting, in which résumés of the work of the sections are furnished.

Section A.

3. *Trans. Faraday Soc.*, **18**, part i, Oct. 1922.

6. *Journ. Soc. Chem. Ind.*

9. Cf. *Comptes Rendus de l'Acad. des Sciences*, 1921, 1922 ; *Journ. de Physique*, Sept. 1921 ; *Les Rayons X—Ouvrage édité par le Comité des Conférences—Rapports*, Presses Universitaires, Paris.

11. *The Times*, Sept. 9, 1922 ; *Engineering*, Oct. 6, 1922 (in summary) ; cf. *The Air and its Ways* (Cambridge Univ. Press, in course of publication).

15. *Observatory*, Oct. 1922 ; *Nature.*

17. *Proc. London Math. Soc.*, **21.**

19. *Observatory*, Oct. 1922 ; *Monthly Notices*, R.A.S., Nov. 1922.

20. *Observatory*, Oct. 1922 ; expected also to be published in extended form in *Monthly Notices*, R.A.S.

22. Expected to be published in *Proc. R.S.* or *Phil. Mag.*

Section B.

3. To be published in *Journ. Chem. Soc.*

10. *Recueil des Travaux Chimiques des Pays-Bas*, Nov. 1922 (part of communication on recent investigations on the substitution in the Benzene Nucleus).

11. *Journ. Soc. Chem. Ind.*, **41**, No. xviii, 384 R—387 R, Sept. 30, 1922.

13. *Engineering*, **114**, No. 2963, p. 472 ; *Journ. Soc. Chem. Ind.*, **41**, No. 18, p. 393 R ; *Chemical Age*, **7**, No. 170, p. 383. Cf. 3rd and 4th B.A. *Reports on Colloid Chemistry* (H.M. Stationery Office) ; *Trans. Chem. Soc.*, **121**, pp. 621, 711, 1101, 1320, 1362, 2161, 2325 ; *Journ. Soc. Chem. Ind.*, **41**, No. 9, p. 147 T.

15. *Journ. Soc. Chem. Ind.*, Oct. 31, 1922. Cf. 'Measurement of Atmospheric Pollution,' *Q. J. Roy. Meteorological Soc.*, **44**, p. 187 (1918) ; B.A. *Report*, 1919, p. 429 ; *Reports* of Advisory Committee on Atmospheric Pollution, 1916-22 ; 'Suspended Impurity in City Air,' *The Engineer*, Oct. 14, 1921 ; 'Smoke Abatement,' *Journ. Roy. Sanitary Inst.*, **42**, No. 2 (1921) ; 'Pollution of the Air by Smoke and its Prevention,' *The Bulletin*, Oct. 1921 ; 'Reduction of Atmospheric Pollution resulting from the use of Gaseous Fuels,' Final *Report* of Public Works, Roads and Transport Congress (1921) ; 'Dust in Expired Air,' *Medical Soc. of London* ; 'Suspended Impurity in the Air,' *Proc. R.S.* **101** A (1922) ; 'Atmospheric Dust,' *Journ. Soc. Chem. Ind.*, Oct. 31 (1922).

Section C.

3. *Trans. Yorks. Geol. Soc.*, **20**, part i (1923).

5. *Trans. Hull Geol. Soc.*, **6**, part iv, p. 244; more fully in *Trans. Yorks. Geol. Soc.*, **19**, part vi (1922).

13. *Trans. Hull Geol. Soc.*, **6**, part iv, p. 238.

Section D.

2. Cf. *Proc. R.S.*, **93** B, pp. 104, 122.

6 (*c*). To be published in *Fishery Investigations*, Series II., 'Sea Fisheries' (Ministry of Agriculture and Fisheries). **6.** (*d*) *Aberystwyth Studies*, **4**, pp. 229-250.

11 (*d*). Included in 'Notes suggestive of Further Work in Herring Investigations,' *Report*, Dove Marine Lab., n.s. xi.

15. Expected to be published in *Q. J. Microscopical Science.*

19. Cf. *Nature*, Dec. 29, 1921 ; *Salmon and Trout Magazine*, Sept. 1922.

20. Cf. *Aberystwyth Studies*, **4** (1922).

24. Cf. *Lancs. and Cheshire Naturalist*, **12**, No. 3 (Sept. 1919), **13**, No. 2 (Aug. 1920).

SECTION E.

2. *Scott. Geog. Mag.*, Jan. 1923. **4.** *Geog. Journ.*, April 1922.

6. Cf. 'Kingston-upon-Hull, a study in Port Development,' *Scott. Geog. Mag.*, May 1919.

7. 'Water and Water Engineering' (*Journ. Inst. Water Engineers*), Nov. 1922.

8. *Scott. Geog. Mag.*, Jan. 1923. Cf. *Electrical Review*, **91**, No. 2339 (Sept. 22, 1922). **10.** *Scott. Geog. Mag.*, Jan. 1923. **11.** Catalogue in preparation.

14. *Geog. Review*, **12**, p. 655 (New York, Oct. 1922).

16. Cf. *Scott. Geog. Mag.*, Jan. 1923 ; article expected in *Geog. Teacher.*

18. To be published in *Scott. Geog. Mag.*

19. Cf. 'Geology of Portuguese Nyasaland,' *Geol. Mag.* **59**, No. 695, May 1922 ; Portuguese Nyasaland,' *Scott. Geog. Mag.* **38**, July 1922.

SECTION F.

2. *Municipal Journal*, **31**, p. 653. Cf. *Organised Produce Markets*, Longmans, Green & Co., London, 1922.

5. *Economica* (London Sch. of Economics).

6. Material to be published in volume on *The Stock Exchange.*

SECTION G.

Papers have been published in *Engineering*, vol. **104** (Sept. 8, 15, 22), as follow : **1** (*a*), p. 350 ; **1** (*b*), p. 293 ; **1** (*c*), p. 307 ; **1** (*d*), p. 309 ; **3**, p. 341 ; **5** (*a*), p. 348 ; **5** (*b*), p. 349 ; **5** (*c*), p. 346 ; **5** (*d*), p. 344 ; **6**, p. 374 ; **8**, p. 375.

SECTION H.

2. Cf. *The Archæology of the Cambridge Region*, to be published by Camb. Univ. Press.

4. *Compte-rendu du Congrès de l'Assoc. française pour l'Av. des Sciences* (1922).

5. Cf. *Journ. Roy. Inst. Brit. Archit.* **29**, Nos. 18-19, p. 553, Aug. 1922 ; *Times* Lit. Supp., Dec. 15 and 22, 1921, pp. 842, 858, and further report to appear at end of 1922, *ibid.*

7. To be published in *Journ. Roman Studies.*

9. Cf. T. Zammit in *Archæologia*, **67**, p. 127 ; **68**, p. 263 ; **70**, p. 179.

10. Cf. *Proc. Univ. Bristol Spelæological Soc.*, **1** ('The Keltic Cavern') ; further material to be published *ibid.* (1923).

11. Part in *Proc. Univ. Bristol Spelæological Soc.*, **1**, No. 2 ; further *ibid.* No. 3, and *Journ. Somerset Archæol. Soc.*, 1923.

12. Cf. *Nature*, July 14, 1921, p. 623 ; *Proc. Prehistoric Soc. E. Anglia*, Dec. 1922.

21. Cf. *Place-Names and History*, Liverpool Univ. Press, 1922.

SECTION I.

1. To be published in *Atlantic Monthly* (1923).

4. Cf. *Journ. Laryngology*, Dec. 1921, Sept. 1922 ; *Nature*, Oct. 21, 1922.

5. *Lancet*, Sept. 23, 1922, p. 685 ; popular account to be published in *Discovery* ; fuller account expected to be published in *Q. J. Medicine*, Jan. 1923. Cf. *Physical Reviews*, **2**, p. 310 (1922).

7 (*b*). To be published as 'Vitamins in Agriculture,' *Bulletin*, Univ. Coll. Reading ; cf. *Biochem. Journ.*, **16**, No. 3, p. 394 (1922).

8, 9. *Neurotherapie*, Dec. 1922 ; *Münchener Med. Wochenschrift*, Dec. 1922 ; *Zeitschr. f. Immunitätsforschung*, 1923 ; cf. *Lancet*, II, 1366 (1921).

12. Cf. *Phil. Mag.*, Nov. 1922.

15. Cf. *Journ. Physiology*, **56**, p. 232 (1922).

SECTION J.

1. To be published in *Anals de l'Inst. d'Orientacio Professional*, Barcelona.

3. *Journ. Nat. Inst. Industrial Psychology*, Apr. 1923.

4. *Machinery Market*, No. 1148, p. 29, Nov. 3, 1922.

8. Cf. 'Education and Industry,' by H. Binns, *Journ. Leeds Univ.*, Jan. 1923; 'Comparison of the Judgments of Children and Adults in the Evaluation of Cloths,' by H. Binns and C. Burt, *Journ. Nat. Inst. of Psychology*, **1**, No. 3 ; 'An Experimental Enquiry into School and Industrial Ability,' by H. Binns and W. Macpherson, *Journ. Experimental Pedagogy*, Mar. 1923 ; 'Industrial Importance of Manual Training in Schools,' by H. Binns, *Wool Record and Textile World*, **22**, p. 689 ; 'Human Factor in the Judgment of Yarn and Cloth,' *Journ. Bradford Textile Soc.*, 1920-21 ; 'Some Experi-

ments in the Measurement of Native Ability and Acquired Skill,' *Journ. Textile Inst.*, **12**, 1.

10. *Psyche*, Jan. 1923.

11. Cf. T. H. Pear, *Remembering and Forgetting*, Chap. xii (Methuen, London, 1922); lectures on 'The Psychological Aspects of Training in Industry: (1) Training the Workers, (2) Training the Management,' *Lecture Conferences* held at Oxford and Scarborough, Sept. and Oct. 1920 (York Printing Co.).

16. Cf. volume on the Sense of Humour in Children, to be published shortly.

Section K.

1. Expected to be published in *Annals of Botany.*

2. To be published in *New Phytologist.*

5. Cf. *Trans. Roy. Soc. Edinb.*, **49**, pt. iii, No. 12 (1913); **53**, pt. i, No. 1 (1921), No. 13 (1922).

8. Continuation of paper 'On the Irritability of the Fronds of *Asplenium bulbiferum* . . . . ,' *Proc. R.S.* B, **93** (1922).

9. Cf. *Development and Properties of Raw Cotton* (London, Black, 1915); 'Existence of Daily Growth Rings in the Cell Wall of Cotton Hairs,' *Proc. R. S.*, B, **90**, 1919; 'Further Observations on Cell Wall Structure as seen in Cotton Hairs,' *Proc. R. S.*, B, **93**, 1922.

10. Cf. Buller, *Researches on Fungi*, vol. **2** (Longmans, 1922).

14 (*a*). Cf. 'Is the Theory of Natural Selection Adequate?' *Nineteenth Century and After*, Oct. 1922; *Age and Area*, Camb. Univ. Press, 1922.

14 (*e*). Cf. J. T. Cunningham, *Hormones and Heredity* (London, Constable, 1921).

15. Cf. 'Physiological Studies in Plant Anatomy; III. The Structure of the Endodermis in Relation to its Function,' by J. H. Priestley and Edith E. North; *New Phytologist*, **21**, pp. 113-139, 1922; 'Physiological Studies in Plant Anatomy; IV. The Water Relations of the Plant Growing Point,' by J. H. Priestley and R. M. Tupper-Carey, *New Phytologist*, **21**, pp. 210-229, 1922; 'The Toxic Action of Traces of Coal Gas upon Plants,' by J. H. Priestley, *Annals of Applied Biology*, **9**, pp. 146-155, 1922.

17. Cf. *Journ. Genetics*, **6**, pp. 237-253; papers to be published in *Journ. Genetics* ('A peculiar type of Variability in Plants'); *Proc. 2nd Internat. Congress in Eugenics* ('A new type of Variability in Plants').

22. Cf. *Sci. Proc. Roy. Dublin Soc.*, **16** (n.s.), pp. 369-434, Feb. 1922; *Notes from the School of Botany, T.C.D.*, **3**, part iii, Mar. 1922.

23. To be published in *New Phytologist* as part vi of 'Physiological Studies in Plant Anatomy.'

25. Expected to be published in *Recueil des Travaux Botaniques Néerlandais.*

26. Cf. 'Preliminary note on the Reproduction of *Rhodymenia palmata* Ag.' in *Annals of Botany*, **37**, No. cxlv (Jan. 1923).

Section L.

7. Reprinted as *Youth in the Universities*, with preface by H. G. Wells; *Nat. Union of Students.*

9 (*b*). *Journ. Education*, Nov. 1922.

10. (*a*) Cf. 'The Sequence of Theorems in School Geometry,' in *Math. Gazette*, May 1922.

10. (*c*) Cf. *Contents of the Fifth and Sixth Books of Euclid* (2nd ed.), Camb. Univ. Press, 1908; *Trans. Cambridge Phil. Soc.*, 1922; *The Theory of Proportion*, Constable, London, 1914.

Section M.

3. Expected to appear in summary in *Journ. Bath and West and Southern Counties Soc.*, and in *Ann. Report*, Agricultural Research Station, Univ. of Bristol.

5. To be published in *Journ. Agric. Sci.*

7. To be published in *Journ. Agric. Sci.*

3. Cf. *Journ. Agric. Sci.*, **12**, part iii, pp. 257-268; J. W. Capstick, 'A Calorimeter for use with large Animals,' *Journ. Agric. Sci.*, **11**, part iv, p. 408; J. W. Capstick and T. B. Wood, 'Progress of Metabolism after Food in Swine,' *Proc. R. S.*, **94** B, p. 35.

SECTIONAL COMMUNICATIONS

ORDERED BY THE GENERAL COMMITTEE TO BE PRINTED *in extenso.*

DISCUSSION ON THE ORIGIN OF MAGNETISM.

Opening remarks by PROFESSOR P. WEISS.

I REGRET that the absence of Professor Langevin deprives us of hearing from him an account of his remarkable work on magnetism.

M. Langevin assumes that the rotation of the molecules, each of which has a magnetic moment, is completely free, and, by investigating according to the methods of statistical dynamics the problem of the orientation of these molecules under the combined influence of an external field and their own thermal agitation, he obtains the law of magnetisation of paramagnetic substances in terms of the field and the temperature. In the region of fields experimentally attainable it reduces to the proportionality of magnetisation and field, and to the variation of susceptibility in inverse proportion to the absolute temperature,

$$I = \frac{1}{C} \cdot \frac{H}{T}.$$

This law was found several years ago by Curie in his experiments on gaseous oxygen. It is only possible to get outside the region of proportionality between magnetisation and field when T reaches a very low value. Recently Kammerlingh Onnes, in studying the magnetisation of gadolinium sulphate, has observed, at 1.5° absolute, unmistakable indications of the approach of saturation.

Langevin's theory may perhaps be compared with the kinetic theory, which deals with the compressibility and dilatation of fluids of low density and leads to the laws of Boyle and Gay-Lussac, $pv=RT$. We know that van der Waals has extended this theory to fluids of great density by adding to the external pressure p an internal pressure which expresses the mutual action of the molecules. I have tried to proceed in an analogous manner with regard to magnetism by adding to the external field a molecular field, $H_m=NI$, proportional to the intensity of magnetisation, and having the same direction. This molecular field expresses the turning action which the assemblage of magnetic molecules constituting a body exerts on any one among them.

The consideration of the mutual action of magnetic molecules is not new. We all know with what success Sir J. A. Ewing has made use of it to explain a whole series of phenomena which, before his work, were obscure; the shape of the curve of magnetisation, hysteresis, magnetostriction, etc. I shall show in the course of this exposition that the forces represented by the molecular field are of an order of magnitude quite different from those invoked in Ewing's theory, and that they explain other phenomena. Moreover, whilst Ewing's forces are mutual magnetic actions, the molecular field is only a magnetic notation for forces which are, in reality, non-magnetic.

If we examine the properties of a substance which, besides having the structure of Langevin's paramagnetic body, possesses the molecular field, we find that the stable state, in the absence of an external field, is not the non-magnetic state, but a condition of magnetisation of finite extent which I have called *spontaneous magnetisation*. And it can be shown that this spontaneous magnetisation has the same numerical value as that of saturation at the temperature under consideration. One must not confuse this saturation with the absolute saturation dealt with by Langevin. The latter corresponds to the complete alignment of the magnetic molecules, whilst the former differs from it by the fact of the thermal agitation of rotation. In a ferromagnetic substance, apparently unmagnetised, the spontaneous magnetisation has different directions at different points, and the non-magnetic state is the result of the mutual compensation of these magnetisations. The action of a field upon a bar of iron thus consists, not of producing magnetisation, but of co-ordinating the spontaneous magnetisations of the different parts by rendering them parallel.

The molecular field theory has given, for the first time, the law of the variation of magnetic saturation as a function of the temperature, and this law has been verified by new experiments with magnetite.

Ferromagnetism disappears at a certain temperature which has been called the Curie Point. Above this temperature the substance is paramagnetic. The molecular field has enabled us to find the law of this paramagnetism; the reciprocal of the coefficient of magnetisation χ is proportional to the excess of the temperature T over that of the Curie Point Θ,

$$\frac{1}{\chi}=\frac{1}{C}(T-\Theta).$$

This law was capable of immediate verification by means of Curie's experiments on nickel, carried out previously. It has since received numerous confirmations from accurate experiments on the ferromagnetic metals and many of their alloys.

If we assume that the coefficient N of the molecular field has three different values in three directions at right angles, we can find an explanation for the remarkable properties of the crystal of pyrrhotine, with its magnetic plane, and in this plane the rectangular directions of easy and difficult magnetisation.

Lastly, two applications of the molecular field in energy considerations. It has long been known that the specific heat of ferromagnetic substances displays an anomaly at the Curie Point. Some would speak of a heat of transformation. But the theory of the molecular field has shown, on the contrary, that the phenomenon consists of a discontinuity of the true specific heat, which, at the Curie Point, falls abruptly to a smaller value. The magnitudes of the discontinuity, calculated from magnetic data and measured calorimetrically, have been found to be concordant.

The recently discovered magneto-caloric phenomenon consists of a reversible variation of temperature which accompanies magnetisation. It is quite different from hysteresis, which is irreversible and always

involves a heating. Any substance when magnetised becomes hotter, and when demagnetised, cools. But this phenomenon is produced by true variations of magnetisation only, and not as a result of the apparent variations due to the change of orientation of spontaneous magnetisation. It is given by

$$\Delta t = \frac{N}{2c \, . \, d} \Delta I^2,$$

where N is the coefficient of molecular field already defined, c the specific heat, d the density, and I^2 the square of the intensity of magnetisation. The phenomenon is only of real importance in the neighbourhood of the Curie Point. Below this point magnetisation is mainly of the apparent type, and above it the values of I^2 quickly become very small. At the Curie Point the effect is far from being negligible; in fact, it reaches for nickel a value of about 1° in fields which are readily attained. The extent of temperature variation calculated by the molecular field theory has been shown to agree with that observed.

The general effect of these results is to show that the molecular field theory is firmly supported by experiment, and these various phenomena enable us to determine the numerical value of the molecular field. For metals at ordinary temperature it is found to be of the order of magnitude 10^7 gauss. Now it is easy to show that in the most favourable circumstances the magnetic field produced by the magnetic moments of the molecules of a ferromagnetic body cannot be greater than 10^4 gauss. It is, therefore, impossible for the mutual actions represented by the molecular field to be of a magnetic nature. It is just a notation for forces of a non-magnetic character, with a symbol borrowed from magnetism. I prefer, in place of the primitive definition given earlier, the equivalent definition

$$H_m = -\frac{\delta U}{\delta I}$$

where U is the intrinsic energy and I the intensity of magnetisation, both reckoned per unit volume. This definition is advantageous in that it does not prejudge the nature of the forces.

In relation to the question to-day under discussion we can thus conclude:

One of the essential conditions for the production of strong magnetism—or ferromagnetism—is the existence, between the molecules possessing magnetic moments, of important mutual actions which are numerically expressed by the molecular field, and are certainly of a non-magnetic nature.

It does not appear to be impossible that the forces may be electrostatic; that, however, is at present a pure supposition.

I would like now to draw your attention to another condition which governs the display of magnetic phenomena. All the theories of magnetism due to Weber, Ampère, Lord Kelvin, J. A. Ewing, Langevin, and others have assumed that the molecules or atoms possess magnetic moments. But it is only since we have been in possession of the kinetic theory and the molecular field theory that we have been able easily

to deduce from experiments on ferromagnetic and paramagnetic substances the exact numerical values of these atomic moments. According to the type of experiment utilised, these moments classify themselves among a certain number of groups. In 1909 M. Kammerlingh Onnes and I measured the magnetisation to saturation for iron and nickel in the neighbourhood of absolute zero. From these first measurements there immediately appears a result of which we shall later on see the generality.

All atomic moments are integral multiples of the same elementary moment, to which I have given the name 'magneton.' There are eleven magnetons in the iron atom and three in the nickel atom at very low temperatures. Other atomic moments emerge from the study of alloys made of ferromagnetic metals, also at low temperatures. These measurements have given indirectly nine magnetons for cobalt, and have led to the discovery of the alloy Fe_2Co, which is interesting because its molecule possesses thirty-six magnetons—a number greater than the sum of those of the constituent metals. This alloy, which at ordinary temperature has an intensity of magnetisation some 10 per cent. in excess of that of iron, has been brought into practical use in the construction of the pole-pieces of electromagnets.

The study of ferromagnetic substances at temperatures higher than the Curie Point has been a new source of magnetic moments. The magnetic moment of nickel for a temperature interval of about 400° has been determined six times by means of independent series of observations made by different observers. They have found 8.03, 7.99, 8.04, 8.05, 8.03, and 7.98 magnetons respectively—*i.e.*, numbers in the immediate neighbourhood of the integer 8. It is worthy of notice that above the Curie Point the atomic moment of nickel is different from its value at low temperatures. This possibilty of the same atom assuming different magnetic moments is a general property.

Investigations with solutions have furnished very many atomic moments. The important researches of Prof. Cabrera and his pupils should be mentioned in particular. It turns out that in the case of the dissolved salts of nickel, for example, there is found the same atomic moment, whatever the concentration may be. This moment proves with great precision to be equal to sixteen magnetons for nickel in the chloride, the sulphate, and the nitrate. The two series of experiments, one in Madrid and the other by Mlle. Bruins in my laboratory, were absolutely concordant. In other cases the atomic moment calculated by means of the paramagnetism of the solution assumes a definite value only at extreme concentrations, whether very weak or very strong. In the interval the atomic moment apparently varies. This arises from the fact that the moment of the metal in the ion, or in the hydrolised molecule, is not the same as in the undissociated molecule. Thus Fe possesses 27 magnetons in very dilute solutions of $FeCl_3$, and tends towards 29 magnetons in very concentrated solutions of this salt.

The study of the magnetisation of paramagnetic salts in the solid state has brought to light magnetic moments characteristic of the type of combination; this also lends support to the theory of the magneton.

Finally, Kopp has recently been able, by making use of the difference of the law of thermal variation, to separate, in the case of platinum and palladium, the ferromagnetism from the underlying diamagnetism; and thus to determine the atomic moments of these metals. He has found multiples of the magneton.

The appearance of atomic moments as integral multiples of the same elementary moment—the magneton—is thus one of the important aspects of magnetic phenomena.

But in order really to explain the existence of this universal moment it would be necessary to be able to link it up with the atom of Rutherford and Bohr and the theory of quanta. Now this theory actually does point to a magnetic moment:

$$\mu = \frac{h}{4\pi} \cdot \frac{e}{m},$$

where h is Planck's constant and e/m is the ratio of charge to mass in an electron. But when the calculation is made it is found that the moment derived is almost exactly five times as great as the magneton. The Rutherford-Bohr atom and Planck's theory thus do point to atomic moments which are integral multiples of the same elementary moment; but the magnitude of this elementary moment differs from that derived from observation. It may, therefore, become a question of finding what modification to introduce into the structure of the atom in order to bring about more complete accordance between theory and experiment.

DISCUSSION ON THE NITROGEN INDUSTRY.

Dr. J. A. Harker.—*Post-War Progress in the Fixation of Nitrogen* (Abstract).—On the statistical side, there is little to add to the Statistical Supplement to the Report of the Nitrogen Products Committee, whilst the rapid fluctuations in the value of the mark make it useless to discuss the cost of production in Germany. The nitrogen problem has assumed great public importance, and some acquaintance with it has even been demanded of schoolboys, although five or six years ago ignorance of the subject was great, even amongst those on whom fell the responsibility for decisions of national importance. The literature on the nitrogen question originates, to a greater extent than is usually the case, from prejudiced sources, so that impartial estimates of the relative values of different processes are rarely found.

When in Switzerland recently, I met a chemical engineer from Poland, who asked me many questions about the Report of the Nitrogen Products Committee. On inquiring of him how he heard of the Report, he answered that his attention had been called to it by the managing director of a well-known firm of constructors of chemical plant in Berlin, who told him that in his view it constituted the only presentation hitherto made of the nitrogen problem in its various aspects, in which the different processes were examined impartially and in detail, and their advantages and disadvantages set forth without prejudice by a competent judicial authority.

The arc process, the oldest method of obtaining fixed nitrogen from the air, consists in burning air by passing it quickly through an intensely hot electric flame. In the best known type of furnace, the Birkeland-Eyde, this flame is a powerful arc, spread out by a magnetic field into a sort of Catherine-wheel, several yards in diameter. A small percentage of the nitrogen and oxygen traversing the path of the arc is caused to combine, and the resulting nitric oxide is ultimately further oxidised to nitric acid or nitrates. This process was commercially operated in Norway in 1904, and in the hands of the original company it has been uniformly successful, and has reached an enormous development. Although the power requirements are very large, it affords the cheapest known method for the manufacture of nitric acid. It has not been sufficiently recognised that the arc process owes its initiation to scientific researches carried out mainly by British investigators. During the War the officials of the Norsk Hydro Company told me that Prof. Birkeland used to recognise frankly that his inspiration was derived from the famous British Association address of Sir William Crookes, and especially from the quantitative experimental work of the late Lord Rayleigh, whose big flask mounted on a wooden stool at the Royal Institution, and provided with a pair of metal poles and an internal potash fountain, is the lineal ancestor of all the great Norwegian plants of to-day. These experiments, employing one or two horse-power, in which Lord Rayleigh carefully measured for the first time the relation between the energy consumed and the amount of nitrogen fixed, pointed the road to all that has since happened in Norway. At Notodden plant is now installed to utilise about 45,000 kw., and in the two great works at Rjukan I saw furnaces in operation employing in all over 200,000 kw., or 270,000 continuous horse-power, this energy being generated at what is almost certainly the cheapest hydro-electric plant in the world. At Rjukan II. a 15,000 kw. steam-operated set has been added to utilise waste steam raised in the boilers employed in the cooling of the process gases, which leave the furnace at about 1,000° C. During the later war period almost the whole output of these enormous works came to England and France for munition purposes.

It is, however, startling to remember that less than 2 per cent. of the electrical or heat energy expended in the average arc furnace is absorbed as chemical energy in the initial oxidation of the nitrogen. The concentration of nitric oxide in the gases leaving the furnace averages only about 1.2 per cent., and many suggestions have been made for improving the efficiency. About fourteen arc plants of various types, mostly of small output, are now distributed throughout the world. Among these I may mention an experimental plant of about 300 kw. capacity which was erected at a munition works at Birmingham on the Kilburn Scott system.

Enriched air has been used on a considerable scale by a company operating works in Switzerland and Germany. The oxygen content of the air is increased to 50 per cent., and the whole operation is carried out in a closed circuit. The product is generally removed as liquid nitrogen peroxide by cooling, instead of being absorbed in towers. Although the increase in yield was considerable, certain serious diffi-

culties were encountered, and several explosions have recently occurred in these plants. The nitric acid works on the Birkeland-Eyde system erected during the War in France are now closed, it having been found necessary for the power employed to revert to its original purpose—railway electrification.

Turning now to the ammonia process, which has often been described, this was originally worked out by Haber and his colleagues, among whom I should specially mention Dr. Le Rossignol, an Englishman born in Jersey, and the late Dr. H. C. Greenwood, the brilliant young investigator whose death three years ago we all deplore. The process was translated into a technical success by the Badische Company, and is now employed on a huge scale at their original works at Oppau, on the Rhine, and at the much larger works recently completed at Merseburg, in Saxony. But to-day this is by no means the only process for the manufacture of ammonia from its elements. From the table you will see that there are now many other synthetic ammonia processes, distributed over the leading countries of the world. In the original German process the scale is large and the gas velocities are low, the reaction vessels being enormous flanged tubes of steel nearly 40 feet long and over 3 feet external diameter, with walls 6 inches thick. In the process worked out independently during the War, at the research laboratory of the Nitrogen Products Committee at University College, much higher gas velocities were employed, giving about twelve times as much ammonia per hour for each litre of space filled with catalyst.

In the plant to produce 11,000 tons of nitrogen annually in the form of ammonium nitrate, erected at Sheffield, Alabama, by the American Government from the designs of the General Chemical Company in 1917-18, activated sodamide was used as catalyst. As the activity, at first fairly high, was permanently destroyed even by small quantities of water vapour, it is not surprising that this plant did not reach the stage of commercial operation. Profiting by the lesson, an American company formed by the General Chemical Company and the Solvay Process Company have erected near New York an improved plant on somewhat similar lines, which has for over a year been producing liquid ammonia for the refrigerator industry at the rate of ten tons per day. The process invented by M. Claude, which operates at the high pressure of about seven tons per square inch, has been recently described in detail. The issuing gas contains about 25 per cent. of ammonia. Multiple stage working replaces circulation, and the dimensions of a full-sized unit plant appear almost absurdly small in comparison with the monumental structures employed in the German process. The last time I was at Montereau M. Claude showed me at work his latest catalyst tube, made, I am pleased to say, by a well-known firm of steel-makers at Sheffield, of a new material which stands up extremely well at high temperatures. It is to the preparation of cheap hydrogen from coke-oven gas that M. Claude is at present devoting his attention. In the Casale process, now working at Terni, near Rome, the hydrogen is generated electrolytically by water-power, and some of it is then burnt with air, producing the required mixture of hydrogen and nitrogen.

FORMS OF THE SYNTHETIC AMMONIA PROCESS.

Country and Date	Process	Operating Pressure Atms.	Approximate space-time-yield kg./lit. cat./hr.	Approx. Percentage Conc. NH_3.	Method of Ammonia Removal	Source of Hydrogen	Remarks
Germany (1913)	Haber - Bosch (Badische Co.)	200	0·3 to 0·5	7–8	(*a*) Cooling. (*b*) Solution in water	Water Gas Catalytic Process	Very large units, low gas velocity. Circulation, Preheating Process in large scale operation
England (1917)	Min. of Munitions (Greenwood and associates)	150	5–20	2–4	Solution with temp. cycle	Do.	Much higher gas velocity, some electric heating. Experimental scale only
England (1921)	Syn. Amm. and Nit. Ltd., Brunner, Mond & Co., Billingham	—	—	—	—	—	Experimental works plant working on a scale of 2 tons per day
U.S.A. (1918)	Gen. Chem. Co. (Sheffield, Alabama)	Under 100	About 0·4	8	Cooling to 30° or 40°	Water Gas Catalytic Process	U.S. Govt. Works, designed for 11,000 tons N. annually. Not now in operation
U.S.A. (1920)	At. Nit. Corp. Solvay Process Co., Syracuse	—	—	—	—	—	Working on a scale of 10 tons NH_3 per day. Circulation
France (1920)	Claude . . .	900–1000	About 5	25	Condensation at atm. temperature	Various	Working on a scale of 5 tons a day. Three stages in series. No circulation
Italy (1922)	Casale (Soc. Idros. Terni)	About 600	—	—	Do.	Electrolytic	Working on a scale of about 4 tons per day. Circulation
Italy (1922)	Pfauser . .	—	—	—	Solution	Electrolytic	—
Norway (1921)	Cederberg . .	—	—	—	—	—	—

Several new types of electrolytic cell have been worked out by inventors in this country and in Switzerland, Italy, and America. Under present conditions, with the high price of coke, hydrogen produced in bulk from cheap hydro-electric power will compete easily with that obtained by other methods.

The cyanamide process, stated by many authorities to be obsolete or, at any rate, obsolescent, had at the end of the War an aggregate capacity in tons of nitrogen distinctly greater than that of any other fixation method. It is true that since the Armistice many of the plants have been closed. The largest of these, capable of producing about 200,000 tons annually, was erected during 1918 by the American Government at Muscle Shoals, in Alabama. The American nitrogen programme, including expenditure on unfinished plant, scrapped at the time of the Armistice, cost no less than $140,000,000, Muscle Shoals alone, exclusive of the hydro-electric scheme now being proceeded with, having cost nearly twice as much as Gretna. At present it is still uncertain whether the Government will lease it to Henry Ford or some other private interest, or will keep it and the Sheffield plant in reserve.

There has been no new development of outstanding importance in the manufacture of cyanamide itself, although detailed improvements have been made; but even under the present difficult conditions cyanamide is still almost certainly the cheapest form of combined nitrogen. In Germany, notwithstanding the recent big synthetic ammonia development, plans are on foot to double the capacity of the great cyanamide plants at Piesteritz. Cyanamide is an unsatisfactory fertiliser for many soils. Examining an old sample of cyanamide, I found that the nitrogen had practically changed over to dicyandiamide. Many efforts have been made within the last few years to convert cyanamide cheaply into some other nitrogen compound, but as yet with small success. An American company is now manufacturing from cyanamide a concentrated mixed fertiliser in the form of mono-ammonium phosphate, sold as "ammophos." This would appear to be a promising material, but too costly for many applications. During the last year independent investigators working in Sweden and Switzerland have succeeded in perfecting processes whereby free cyanamide is prepared by the action of carbonic acid on calcium cyanamide, and is subsequently converted into urea. In the Swiss process, excess of the sulphuric acid employed in the second part of the transformation is used to act on phosphate rock, which is changed to mono-calcium phosphate, the final product being a neutral body known as phosphazote, having its nitrogen content as urea and its phosphorus in the water-soluble form, usually with 11-12 per cent. nitrogen and 11-12 per cent. available P_2O_5. The cost of manufacture is said to be by no means high, and the substance has no deleterious action on the skin or on the bags in which it is packed. It has been manufactured on a fair scale for about six months, the product going mostly to France for vine culture. These two processes, which are apparently in course of rapid development, may prove to be a means of rehabilitating cyanamide as a product of fixation.

After the Oppau explosion mixed salts containing ammonium nitrate will probably be under the ban of many fertiliser dealers, although the substances may be perfectly safe if their possessors are not sufficiently careless or idiotic to attempt to remove them by the stimulus of a big blasting cartridge.

I can do no more than refer to the cyanide process, which is by far the oldest fixation method and still attracts many investigators. A British company is continuing its experiments at Birmingham, and in America cyanide is being made on a considerable scale from cyanamide as a source of hydrocyanic acid for plant fumigation. The process depends on a number of complex chemical changes, and further research is required on the fundamental reactions involved. Investigations with this object are being undertaken in the Fixed Nitrogen Research Laboratory of the American Government.

In 1913 Germany consumed about 200,000 tons of nitrogen, of which about 110,000 tons was imported in the form of Chile nitrate. The bulk of this was used in agriculture for intensive cultivation. From May 1921 to April 30 this year the smaller Germany of to-day consumed 290,000 tons, without the demands of her agriculturists being fully met. The whole of this increased total was produced within the country. Though the German Government pleads bankruptcy, the Badische Company appears to have had little difficulty in finding money to repair the enormous damage caused by the great explosion at Oppau, and these works are now on the point of being again ready to produce their full rated output. At the end of the present year Germany will have at her disposal an internal capacity for the production of about 500,000 tons annually, and will be entirely independent of all importation. In case of war, she will thus be assured of the basic materials for a gigantic production of munitions, together with enough fertiliser to enable her to grow a very large share of her own food.

Mr. J. H. West.—*Raw Materials for Synthetic Ammonia: The Manufacture of Hydrogen and Nitrogen.*—There are two main sources of commercial hydrogen, coal and water. Hydrogen from water by electrolysis is very pure, but the capital cost of the plant is high, and the consumption of electric energy is so great that unless cheap water-power is available the cost of production is prohibitive.

Many coals contain about 5 per cent. by weight of hydrogen, equivalent to 21,000 cu. ft. per ton of coal. On distilling coal in retorts or coke ovens about one-quarter of the hydrogen is set free, the other three-quarters remaining in the tar, oils, and ammonia. Coke-oven gas, where available, is an excellent source of hydrogen, which can be separated from it by M. Claude's ingenious process of liquefying all the gases present except the hydrogen, after having removed the carbon dioxide by absorption in water under pressure

The interaction of steam and hot coke gives water-gas, which contains about 50 per cent. hydrogen and 42 per cent. carbon monoxide, the yield being about 55,000 cu. ft. of gas per ton of coke.

The carbon monoxide in water-gas may be caused to react with steam in presence of a catalyst, producing carbon dioxide and hydrogen, according to the equation $CO + H_2O = CO_2 + H_2$.

The process which has been worked out by Dr. A. Jaques and J. H. West combines the distillation of coal in a retort, the formation of water-gas from the resulting coke, and the conversion of the carbon monoxide produced in these two operations into carbon dioxide and hydrogen by reaction with steam in presence of a catalyst, in one apparatus, thus giving the maximum possible yield of hydrogen from a ton of coal. The hydrogen present in the coal is practically all liberated by passing the crude coal-gas through a hot zone in the producer, so that all tar, oils, and hydrocarbons, such as methane, are cracked or split up into hydrogen and carbon, the carbon reacting with steam to form water-gas. The process is carried out in a modified form of Tully complete gasification plant, and the only products are gas and ashes. Nitrogen can be made in an ordinary liquid-air plant, the separation from oxygen being effected by fractional distillation.

Another method is to burn out the oxygen of air with hydrogen. In other words, a measured quantity of air is added to the hydrogen and the mixture is passed over a catalyst, so that combustion takes place quietly, and a mixture of hydrogen and nitrogen in the desired proportions results.

In the Haber process water-gas is mixed with air-producer gas, so that sufficient nitrogen remains in the mixture after treatment for conversion of the carbon monoxide, the final adjustment to the exact proportions being made by adding a little pure nitrogen obtained from liquid air.

Mr. C. J. Goodwin.—*The Häusser Process of Nitrogen Fixation.*—The commercial success of the manufacture of synthetic ammonia in conjunction with the Ostwald process of obtaining nitric oxides and nitric acid by combustion of the ammonia have created the impression that, in the near future, such ammonia will be the principal raw material for nitric-acid manufacture. Colour is lent to this view by the economic failure of the arc process unless under exceptionally favourable conditions, and by the price conditions obtaining in the nitrate of soda and sulphuric acid markets.

Economically, and on general principles, such an assumption seems unsound provided there is an assured output for all the synthetic ammonia produced, because even if the loss on conversion is small, it is irrational to make ammonia in plant involving high capital costs if nitric acid could be made direct in cheaper plant at the same cost. Recent developments in the Häusser process have brought the commercial realisation of this statement within sight.

The details and theory of the Häusser process were briefly discussed, and it was shown that the impending trials with a 1,500-litre bomb are likely to lead to commercial yields. Modern alloys and stainless steel have assisted in overcoming difficulties in wear and corrosion, and the volume of the absorption-tower system has been reduced to one-sixtieth

of the normal by absorption under 3 atmospheres pressure in chromium-nickel-steel towers.

Although bombs have proved perfectly satisfactory in operation, there remains the possibility of substituting a gas-engine, or preferably a modified type of Humphrey pump, for the explosion bombs. It is possible that a greater percentage of the calorific value of the gases or liquid fuels used could thus be converted into useful energy. Such plant would necessarily be of a more complicated nature than a bomb installation, and it is doubtful whether there would be any real gain on balance. Finally, the importance of using gases of higher calorific value as giving higher temperatures and yields is emphasised, and methods are indicated by which coke-oven and similar gases can be enriched to meet these conditions. It is suggested that Häusser plants can be usefully developed either as adjuncts to synthetic ammonia plants, at coke-oven plants, or in places where liquid fuel is cheap and the cost of making nitric acid is relatively high

Dr. E. B. Maxted.—*Some Aspects of the Relation between Water Power and Nitrogen Fixation.*—Nitrogen fixation processes involve two economic factors, power and material, the relative importance of which will determine not only the most suitable site in a given country, but also the practicability of the method under given geographical and mineralogical conditions. For the arc process the availability of cheap electrical energy is the determining factor, whilst the ammonia process is comparatively independent of such conditions, cyanamide occupying an intermediate position. In 1914 some 50,000 tons of British anthracite were exported to Norway for the manufacture of carbide. Under British conditions, except for the manufacture of nitric acid as such, probably only the cyanamide and the ammonia synthesis processes are practicable. The ammonia process would be almost independent of power but for the fact that the hydrogen may be manufactured electrolytically as an alternative to the process in which water-gas is prepared from fuel.

The Water Power Resources Committee has reported on resources in Great Britain estimated to be capable of producing some 250,000 kw., exclusive of the Severn Estuary scheme. Of the ten Scottish sites reported on favourably only one was estimated to produce over 30,000 kw., and of the five Welsh sites only one over 5,000 kw. Sites producing less than 10,000 kw. are not suitable for cyanamide, whilst the minimum for the synthesis of ammonia has not yet been determined. An average figure for the cost of power at the Scottish sites would be about 30*l.* on a pre-war basis for each kw. Operating and other costs should not be more than 3 per cent. of the capital cost. Several of the more suitable sites should produce power for about 4*l.*-5*l.* per kw.-year, the cost at certain Norwegian plants being 2*l.*-3*l.* under pre-war conditions. A figure of 1*s.* 7*d.* per 1,000 cubic feet is obtained for the cost of hydrogen with power at 5*l.* per kw.-year, a figure comparing not unfavourably with the cost from fuel, although nothing has been allowed for the value of the oxygen produced. The electrolytic process, how-

ever, requires a large floor space and a multiplicity of cells, but larger units are now being introduced, and there is room for investigation in regard to new uses for oxygen.

Assuming a station having 10,000 kw. available, some 5,000-5,500 tons of nitrogen could be fixed per year, either as ammonia or as cyanamide, but the former process would yield some 33,000 cubic feet of oxygen per hour, whereas the cyanamide would require coal and lime to be brought to the site. Against this is the greater simplicity of the cyanamide process, and the necessity of fixing the ammonia by an acid, unless partly converted into nitric acid or used as anhydrous ammonia or aqueous solution.

Mr. E. Kilburn Scott considered that the arc process still had many advantages, and that Scottish plants could produce power at 4*l.* per kw.-year. The arc process was the only one capable of utilising off-peak power. Calcium nitrate was the most efficient of all fertilisers.

IMPERIAL CITIZENSHIP.

By The Rt. Hon. Lord Meston, *K.C.S.I.*

There are two aspects from which it is possible to approach Imperial Citizenship—distinct, but supplementary to each other, and in no sense antagonistic. From one point of view Imperial Citizenship is an emotion and an ideal, the foundation and essence of patriotism. In Patriotism you have the white flame which blazes out in protection of country or empire; in Citizenship you have the steady glow which warms men's hearts to a pride in their heritage and to a determination to do their share in making it still more worthy of living for or dying for. The ideal is for every member of the community to qualify himself or herself for true citizenship—that function which Aristotle defined as a partnership in the legislative and judicial power of the State. Thus qualified, the body of citizens would form the perfect State.

It is not my purpose to attempt to describe the strides which in our own English-speaking lands are being made towards this ideal, or to touch on the lines along which further advance may develop. It is not on this aspect of the question that I propose to dwell further than to express the fervent belief that Imperial Citizenship, wisely taught to our young and prudently guided in our adults, is capable of becoming a power for the regeneration of the world. The ideal has been temporarily dimmed by the reactions of the War and the painful readjustment of social conditions which is now in progress over a greater part of the world; but if we in this country and in our great self-governing Dominions have any faith in our Imperial calling, we must unite in every effort to establish it as a religion for the future.

The other aspect of the question, and that to which I wish particularly to invite your attention, is Imperial Citizenship as a status. From this point of view we are faced by problems which can never be absent from the thoughts of any man who has had to handle the issues of practical administration. It will thus be no small satisfaction to

many that the British Association has found itself able to include the subject in its programme, for few questions of its kind are more complex, and any light which can be thrown upon it to-day will be valued as a contribution to the work of consolidating and strengthening our Empire State.

The complexity of the topic is at once apparent when we search for a definition of the status implied in Imperial Citizenship. ' Citizen ' *per se* has a wider connotation than ' resident.' It suggests certain privileges and certain responsibilities, enjoyed and shared in common by all who call themselves citizens. A citizen, as we understand the term, has a right to enter any part of his State, and has when resident the same rights to live, to earn a livelihood, to be protected by the laws, to vote for the Legislature and to sit in the Legislature, on the same conditions as his neighbours. He is also required to obey the laws, to pay taxes, and to share in the defence of the country on the same conditions as his neighbours. This, you will probably agree, is a minimum statement of the rights and duties of a citizen in any area in his State, whether he was born in that area or migrated from another part of the same State. It provides, in other words, a working definition of citizenship as a status.

Is there anything which can be defined in corresponding language if for ' State ' we substitute ' British Empire ' ? We know that in practice there is not. A Maori or a Punjabi coming to Hull would be admitted, and would acquire the British citizenship enjoyed by all other natural-born subjects of the Crown in this city. But if he went to certain other parts of our Dominions he would not be allowed to acquire the same status as his neighbours; he might even be refused admission. Similarly, a native of Hull, migrating to Toronto or Melbourne, would soon find himself in possession of the same civic rights as the established residents of those cities; but if he went to the Transkei territories of Cape Colony he would have to accept certain special disabilities; and if he went to India he would be ineligible for certain privileges open to Indians. For these various restrictions there are different reasons, and in every case an explanation. But I am not concerned for the moment with their reasonableness or otherwise; I am merely making the point that Imperial Citizenship, as a status of universal and uniform validity throughout the Empire, does not exist. Its sphere is subject to large reservations, geographical and ethnical.

In the circumstances, how could it be otherwise? The British Empire has been described as a great slice, like a gigantic geological section, cut through the whole social and racial stratification of the world, the various strata representing different types of civilisation, bewildering in their variety. At one end of the section is our own Anglo-Saxon type. Adjoining it are the débris of Asiatic cultures, far more ancient than our own, but also more rigid and less progressive. Then comes the theocratic mass of Islam, heterogeneous to a degree, but at one for the Koran and its sword. And so on, until we ultimately traverse the primitive society of the Bantu races in Africa, and reach at the further limit of the section the pure barbarism of the Australian aborigines or the Bushmen of the Cape.

In such a medley any common standard of civic consciousness or rights or duties is clearly unthinkable. And the most distant avenues of approach to a common platform are barred by obstacles which at present appear unsurmountable. There are antipathies of all sorts, some natural and inevitable, others unworthy, which debar two types of civilisation from assisting each other to a common civic status. There is sheer prejudice—the *hubris*, it may be, of a conquering race, or ignorance and insularity. There is often the natural resentment of a community, weak either in itself or in comparison with the task it has undertaken, for example, in settling a new country—its natural resentment against the competition of another community. Among the higher and older types of culture there is a horror of miscegenation and an instinctive erecting of barriers against it. In a hundred tangible and intangible ways the opportunities of working towards communal standards of life are refused, even if the capacity for achieving them were much greater than in fact it is. But the essential truth to which we must always return is that the uniform and universal status of Imperial citizenship is unattainable so long as there are grave divergencies of civilisation applied to the ordinary observances of life.

All this, it may be said, is merely wrapping up in abstract phrases the concrete fact that certain white communities in the Empire will not permit members of other or coloured communities to live alongside them on terms of civic equality. I agree; but its purpose is to emphasise the radical nature of the problem of the extension of Imperial Citizenship, and the futility of any ready-made solution. For the contrary view appeal is still made occasionally to the edict of Caracalla, in which, by a stroke of the pen, he conferred the Roman citizenship upon all the free-born inhabitants of the Roman Empire. Historians have, of course, long discounted the value of that theatrical coup, which apparently had for its sole object the replenishment of the Emperor's treasury by making the provincials amenable to taxation from which as subjects they had been free; Gibbon, with his usual insight, describes it as conferring 'the vain title and the real obligations of Roman citizens.' This is clearly not a precedent for us to follow. Nor is there any permanent value in a more modern solution, adopted by our own Imperial Conference in 1917, the principle of reciprocity. It was put forward to meet the grievances of Indians regarding their position in the self-governing Dominions; but, apart from the fact that it breaks down when applied to the Crown Colonies, it is obviously a mere temporary palliative, and a dangerous palliative inasmuch as reciprocity may at any time degenerate into retaliation.

And yet the problem, I would repeat, is of high importance. It is also of considerable urgency, for claims to civic status are constantly being pressed by or for communities from whom it has been withheld. Such claims are likely to become more insistent as calls are made on those communities for common services or for conformity with common standards. And they have now a permanent basis in the growth of racial consciousness, in what Mr. Lothrop Stoddard calls the rising tide of colour. Urged frequently, it may be, with more appreciation of the rights than of the duties of citizenship, they are meant primarily

as a protest against implied racial inferiority, as an assertion of racial self-respect. Unless we can find some means of handling them in that sense, I apprehend that the result will be increasing embarrassment in our task of Imperial unity. It will certainly be a growing lack of spontaneity on the part of the claimants in their response to future Imperial calls upon them.

An earlier and a nobler Roman than Caracalla had envisaged a similar problem. When Julius Cæsar turned his marvellous genius to the construction of Imperial Rome, to the conversion of the City State into a Mediterranean Empire, he laid his plans on wider and more generous lines than were revealed to any of his successors. His policy, as interpreted by Mommsen, was to secure ' unity in those institutions which express the general life of nations—in constitution and administration, in religion and jurisprudence, in money, measures and weights.' After a lapse of 2,000 years it would not be easy to improve on this catalogue of the essentials of Imperial unity. Some of the items are not of the same importance as in the ancient world; diversity of weights and measures has been rendered innocuous by commercial ingenuity, and diversity of monetary standards has yielded to the agreeable subtleties of exchange. But unity of constitution and administration, of religion and jurisprudence, remains the ideal foundation for a common citizenship. Religion, indeed, though some of us are old-fashioned enough to consider it the most important of the four, must for our present purposes be left out of the picture. The official adoption of a new faith presented little difficulty to the world of Julius Cæsar's day, and the Roman pantheon was often opened, by way of settling theological controversies, to the adoption of gods from another creed. Religious compromise is not so easy in our days; but in place of absorption we have learned tolerance, and the forcible conversion of subject races has gone out of fashion. In laying out the lines of Imperial Citizenship, therefore, we must assume that Christianity will work independently of the State. We must also aim at a scheme of civic duties which can be fulfilled and civic rights which can be exercised without prejudice to, and without being prejudiced by, the religious practices of the individual citizen.

May we take it, then, that the qualifications for full Imperial Citizenship are (1) the attainment of a similar type of constitution, (2) submission to a uniform system of administration, and (3) the acceptance of a common code of jurisprudence? I think it will be found that these underlie and support the whole sphere of civic rights and duties which we have in our mind. For by a common jurisprudence we mean not only obedience to the same set of laws, but a ready support to the authority which enforces them; we mean equality of all men before the laws, and equal justice in their execution; and we mean a standard of commercial and public morality which the spirit of the law inculcates, though its letter cannot always impose it. Similarly, by a uniform system of administration we imply the acceptance of such regulation of the incidents of everyday life as the general sense of the community demands, particularly in regard to education, industry, sanitation, and public health. And by unity of constitution I conceive that we should

expect a reasonable adherence to the theory and practice of democracy, so far as we have developed them in those parts of our Empire which have made most progress towards political freedom.

If these are the qualifications for admission to full Imperial Citizenship, it is clear that some of the members of our great Imperial federation have a long way to travel before they can possibly acquire them. Some, indeed, cannot hope to reach them within any measurable period; and, so far as we can foresee, there must remain certain classes of our fellow-subjects whose civic status must be imperfect and limited in any part of the Empire. But there are other and far more numerous and important classes which are nearer the desired standard, and the task to our hand is to help them on towards the goal. I thus come to the particular angle from which I was invited to participate in this discussion as the representative of India. And I make no excuse for taking India as the outstanding type of those higher races to whom the status of Imperial Citizenship is a question of practical politics and immediate interest. It is the stratum of our Imperial section of the world which is nearest our own in its civilisation, traditions, and philosophy; and India's commanding position in our commerce and foreign policy raises the question of its status into the first rank of importance.

The issue, it must be admitted, has only recently become acute. Since India became a part of the British Empire it has enjoyed little time or opportunity for anything outside its own borders. It long accepted the position of tutelage, and left us to look after its international concerns. It was fully occupied with its own domestic affairs, with its recurring sectarian troubles, and its own reachings-out after political liberty. Poverty and ignorance have played their part in checking any wider outlook or aspirations. For the small minority who could rise above those crushing handicaps, the preoccupations of industrial advance and constitutional change left little space for Imperial issues. Unfortunately, too, some of the earlier points of contact with those issues originated, so far as India was concerned, in episodes of a secondary and sordid character; for the first stirrings of an Imperial consciousness were aroused by the grievances of Indians who had emigrated as indentured labourers to our sugar-growing Colonies. Disclosures of the treatment which many of those emigrants received in Fiji were certainly not calculated to encourage a sense of citizenship. They evoked bitter resentment in India against our Colonies, and discredited the whole system of emigration under contract. Then followed the grievance of differential legislation, municipal and otherwise, against Indians in South Africa. This was nothing new, for I remember, as far back as 1906, talking with a group of Sindhi traders in the Pretoria market-place, and being assured by them that they had been better off under the Kruger régime than they were under the British. This feeling grew until the belief became general in India that helotry, and not citizenship, was the status designed for Indians in several of our British Possessions.

The spreading agitation on the subject was silenced by the War. With it there came a spontaneous outburst of loyalty, a temporary shelving of grievances, and—among all the best elements in India—a

claim to share in our Imperial obligations and dangers. When this claim was acknowledged, when Indian armies and many millions of Indian gold were thrown into our military resources, the whole question necessarily rose to another level. Indian soldiers were placed on an equality in the field with their British and Dominion comrades. Why, asked Indian thinkers, should not the same principle of equality be extended to the relationships of peace? Why, in particular, should not India participate in the new world of freedom and justice and emancipation for the weaker nations and self-determination, which we were fighting to establish? These questionings survived and grew louder when war was over. British statesmen answered them by certain formal acts and declarations of recognition of India as a partner in our Imperial federation. She was admitted into the League of Nations; her representatives signed the Treaty of Versailles; her nominees were included in the Imperial Cabinets and Imperial Conferences which, originating under the stress of war, have been continued as a permanent feature in the unwritten Constitution of our Commonwealth.

When Indian politicians, however, came to translate those ceremonial courtesies into the terms of practical citizenship, they found themselves face to face with a totally different interpretation of India's status in certain of the Dominions and Colonies. The test case was that of Kenya (British East Africa), where definite disabilities had been imposed on Indian settlers—segregation in the towns, a refusal of the franchise, and a refusal of proprietary rights in certain areas. This particular case is still, so to speak, *sub judice*, so that no opinion need be expressed upon its merits. And from the Indian point of view also discussion is in a sense suspended, as the wise course has been taken of sending Mr. Sastri, one of the most judicious and brilliant of India's public men, on a mission round the Empire to urge upon the various Dominion Governments the claims of Indians to citizenship. It may be assumed that the whole position will be brought into its proper perspective when he returns and reports the results of his negotiations.

Meanwhile, and without anticipating Mr. Sastri's conclusions, I think we may usefully reflect on certain considerations—considerations, it may be, of expediency, but none the less cogent—which make for the earliest possible admission, under proper conditions, of India into the charmed circle of Imperial Citizenship. At the moment, unhappily, we must treat her two great communities, the Hindus and the Mahomedans, separately. Think of their respective outlook on the world beyond India, whether within or without the British Empire. The Hindu theory of life in its extreme form, as preached by Mr. Gandhi and cherished by a large section of the orthodox, is rooted in a philosophy which finds all that is necessary for man in an archaic and exclusive social system, and lays a complicated embargo on all social relations with the rest of mankind who are not born into that system. Fortunately, men are generally better than their philosophies, and there is much tolerance and compromise in Hindu practice. But the forces of reaction are never at rest, and there is constant pressure on the Hindu mind to retire within the ancient battlements of Hinduism, and to exclude all traffic with the West or its methods of human organisation.

If this movement should ever prevail, our work in India would be largely undone, and her Imperial value greatly impaired. Against such a calamity there are several lines of insurance, but the extension of Imperial Citizenship is certainly not one of the least promising.

Turn next to the Moslems of India. Among them, as among their co-religionists in Egypt, Arabia, and Persia, there is a definite movement towards the solidarity of Islam and the weakening of any temporal allegiance to non-Islamic powers. It has taken various forms, and to-day we know it best in India under the guise of the Caliphate agitation, the embers of which may flare out again with the recent successes of Turkey in Asia Minor. Anti-Christian bigotry is never wholly absent from it, but so far as India is concerned its underlying motive is, and has always been, a craving for some wider nationality than a country in which Hinduism is in the ascendant both numerical and intellectual. While, therefore, there can be no rest till the Mahomedans of India are persuaded that it is their country rather than some vague pan-Islamic paradise, it will ease the situation enormously in the meantime if we can offer them that larger horizon for which they long, not in a visionary world-empire of Islam, but in the real world-empire of which they are already members.

The extension of Imperial Citizenship to India is thus more than a step in social progress; it is a paramount political necessity. At this crisis of its fate India needs guidance and inspiration—the guidance of an ideal which will outshine the will-o'-the-wisps now hovering near its path, the inspiration of an emotion nobler and more humane than some of those which now distort its vision. We can provide the ideal and the emotion in the conception on which this paper is based, if we can offer the status in which they are generated.

The difficulties are far from negligible, and it would be foolish to underrate them. It cannot be pretended that India as a nation has attained the qualifications, if we enumerated them fairly, which are required for full Imperial Citizenship. A large and growing number of individual Indians are fully qualified, but it is not with them that our Dominions or most of our Colonies are familiar. The emigrants, especially to our tropical and sub-tropical possessions, are largely drawn from lower and less desirable classes, and there are complaints of their inability to conform to sanitary and other standards which the local authorities find it necessary to enforce. Behind these obstacles there generally lie the clash of economic interests and the determination of a sparse white population not to be swamped by coloured settlers.

What, then, is to be done? While we recognise valid objections to the indiscriminate extension of the privileges of citizenship to Indians, how are we to bring those objections home and to satisfy India that we are not actuated by selfish prejudice or racial *hauteur*? While we assert our willingness to open the doors of our citizenship to India at the earliest possible moment, how are we to persuade India of our sincerity and to stay her impatience and her suspicions? These are the pressing questions before you and me to-day; and by way of contribution to answering them I suggest (at least, as a basis for discussion) that our course is a threefold one.

In the first place, in those parts of the Empire where the question of Indian emigration and settlement is controversial there should be at once a perfectly frank discussion of the issues between India and the Dominion or Colony concerned, and, if necessary, an inquiry by an impartial tribunal, such as a Royal Commission. Much good, and comparatively little harm, will come from a full and open examination of both sides of the question, each party being given a chance of appreciating the other's point of view, and the avenues to understanding and compromise being explored on the spot. I am sure that anything is better than the present long-distance bombardments between the India Office and the Colonial Office, with the uneasy feeling that the British Cabinet could stop the action if it wished. With all the facts and all the sentiments and all the prejudices (if you like) before them, a competent Royal Commission could suggest lines of negotiation, if not a settlement, which would allay the growing acerbities of the situation. It may quite well be that, for emigrants into certain areas, the status of citizenship must be deferred or limited or restricted numerically. Circumstances exist in which such a step would be perfectly reasonable, and if taken with India's consent would in no sense be derogatory to her self-respect or to steady progress towards full achievement.

In the second place, those of us who believe in Imperial Citizenship as a force in the world's progress will do well to see to it that it is preached among ourselves, not as a doctrine for the white races alone, but as an evangel for all the higher strata of civilisation in our Commonwealth. In this, as in so many other matters which are vital to human harmony, we have always to be at the difficult task of trying to 'get into the other man's skin.' We have to remember, and to convince others, that where the status of our Imperial Citizenship is envied and sought for it is a gift which should be ungrudgingly given if at all, for it will surely bring its own rewards. Of these, an increased loyalty is only one; there are others, more subtle and not less valuable—an upward growth towards our Imperial standards, and a steady elimination of such national weaknesses as are incompatible with our Imperial ideals. These are worth some temporary sacrifice and discomfort on our part.

In the third place, returning more particularly to India, we have a mighty task of co-operative effort before us in that country. I am not here to talk politics, but you will probably agree with me that, in her new Constitution, and on the peculiar terms in which we have undertaken its development, India has been placed in the perfectly unique position of being invited to choose her own future, and of being assured protection while she is engaged in working it out. There is a section of Indian opinion—to whose reactionary doctrines I have already referred—who profess the belief that India can move, without our aid, towards her own national organisation. There is another section which, if properly supported, will (I believe) in the end prevail. It looks to us for assistance and co-operation in a problem for which its own experience is not yet adequate. That problem is neither more nor less than the fulfilment of those conditions for full Imperial Citizenship which we were considering at the outset—just and uniform laws, a public spirit which actively co-operates in their enforcement, a standard

of commercial and public morality which will make India respected among the nations of the world; a steadily rising standard of efficiency in education, public health, and industrial life; a growing acceptance of democratic principle and practice. The edifice should be an imposing one, for the country has all the makings of greatness and the material is vast. The foundations are there, in India's own best traditions and in our labours of the last two centuries. We have now got to help in the superstructure. The duty is one that calls for a more intelligent knowledge of India, and a closer interest in her affairs, than have always obtained in the past, either in this country or in the Dominions. Stimulated by that knowledge and that interest, we have it in our power to inoculate our own people with a sense of their responsibility for India. It must no longer be the plaything of political parties, or the stalking-horse of cranks. It must be induced to feel that the thinking British public is committed to assist in its advancement towards a real, and not a merely ceremonial partnership in our Imperial federation. In this way I believe we shall keep it steadfast on the road to Imperial Citizenship.

In opening my story I proposed to approach our subject from the point of view of status rather than of emotion. In dealing with India, I am conscious of having occasionally confused the two aspects. The truth is that they are inseparable. Without the status, we need not look for the spirit. Give the status, and it is our earnest belief that the spirit will abound, for it is the spirit that will make us live. Our Empire has not been built up on the blood and toil of subject-races; it does not rest on tribute, and it will grow in greatness only so long as it abides by the mighty principles on which it has been nurtured in the past, only so long as it remains an association of free peoples, working out their own development each in its own way, but united in a form of brotherhood which will transcend differences of colour and creed, and will find its expression, in an ever-increasing measure, in terms of an Imperial Citizenship.

CORRESPONDING SOCIETIES COMMITTEE.

Report of Committee (Mr. T. Sheppard, *Vice-Chairman*; Dr. F. A. Bather, Mr. O. G. S. Crawford, Professor P. F. Kendall, Mr. Mark L. Sykes, Dr. Clarence Tierney, Professor W. W. Watts, Mr. W. Whitaker, *and the* President *and* General Officers). *Drawn up by the General Secretaries.*

The matters referred to the Committee by the Council of the British Association and reported to the General Committee at the Hull Meeting were considered by the Hull Conference of Delegates (of which the proceedings are appended) and by the Committee itself at Hull.

In accordance with the resolutions adopted by the Council, the Conference of Delegates has invited the scientific societies of Liverpool and district, on the occasion of the British Association's visit in 1923, to consider what further provision, if any, is desirable for co-operation between them for the advancement of science; as, for example, for scientific research, for the discussion of regional problems, and for the publication of results.

The Committee, in accordance with the Council's Memorandum (§ 3), has co-opted the following representatives of the scientific societies of Liverpool and its neighbourhood to assist in preparing the programme of the Liverpool Conference of Delegates :—

Prof. P. G. H. Boswell, Liverpool Geological Society.
Prof. W. J. Dakin, Liverpool Biological Society.
Prof. P. M. Roxby, Liverpool Geographical Society.
Dr. H. F. Coward, Manchester Literary and Philosophical Society.
Prof. F. E. Weiss, Manchester Literary and Philosophical Society.
Mr. T. W. Sowerbutts, Manchester Geographical Society.
Mr. W. H. Barker, Manchester Geographical Society.
Mr. J. S. Broome, Warrington Literary and Philosophical Society.

The Association of Science and Arts Societies for Gloucester and District, formed in 1921, has submitted to the Conference of Delegates an account of its own organisation, and made the valuable suggestion that local Societies should be encouraged to make such arrangements with the nearest University or University College as may ensure that full use is made of each Society's membership, premises, and collections in connection with University Extension courses. The Gloucester Association has already made such arrangements with the University of Bristol.

The Committee recommended to the Conference of Delegates at Hull that the Kent's Cavern Committee should be discharged, and that the contributions received by this Committee towards a fund for the purchase of Kent's Cavern be returned to the donors, as there is no immediate prospect of purchase. This recommendation was adopted by the Conference, and the Corresponding Societies Committee was asked to keep itself informed of any further excavation which may be projected by the present owner of the cavern.

The Committee submits an application for a grant of 40*l*. for the preparation of its Report and the usual Bibliography. Proposals for the improvement of this Bibliography in conjunction with other bibliographical projects have been laid before the Committee, and will receive its careful consideration.

CONFERENCE OF DELEGATES OF CORRESPONDING SOCIETIES.

HULL, 1922.

The Conference met in the Council Chamber of the Guildhall on Thursday, September 7, at 2 p.m.—Mr. W. Whitaker, F.R.S., in the chair—to consider matters referred to the Conference by the Council of the British Association, as follows :—

(*a*) To consider what steps should be taken, in accordance with the recommendation of the Committee for Corresponding Societies in 1883, to induce local societies to group themselves round local (*i.e.* district) sub-centres (i) for the interchange of information, (ii) for the more economical publication of the results of research.

The constitution and procedure of the Yorkshire Naturalists' Union was described by Mr. T. Sheppard (Hull) and Mr. T. W. Woodhead (Huddersfield); that of the Gloucester Association of Science and Arts Societies in a communication from Mr. J. H. Beach (Cheltenham), read in his absence by the Secretary; and that of the South-Eastern Union of Scientific Societies by Dr. C. Tierney (Croydon). Mr. A. W. Oke (Brighton) illustrated the difficulties of maintaining serial publications at the present high cost of printing, and asked how this affected the status of 'Associated' Societies. It was explained that regular publication was not required to ensure these Societies' status under the rules (ch. xi. 2. i.). Mr. O. G. S. Crawford (Society of Antiquaries) urged the pooling of local publications within large regional limits, and the concentration of the Societies' resources on publication of scientific results and on collaborated bibliographies. Dr. F. A. Bather (Museums Association) desired greater uniformity and precision of date and paging in reprints of articles from serial publications.

Arising out of this discussion, the following motions, proposed by Mr. O. G. S. Crawford (Society of Antiquaries), were referred to the Corresponding Societies Committee for consideration and reference to the second session of the Conference : (1) 'That steps be taken to publish a single bibliography, at regular intervals, of articles dealing with the archæology of the British Isles'; (2) 'That when a district is worked by more than one society there should be only one joint journal published in that district.'

(*b*) To consider whether the Delegates sent to the Conference might be authorised to act as local representatives of the British Association in their respective districts.

Questions were asked as to the services which such local representatives might be expected to render to the Association, and the matter was referred to the Corresponding Societies Committee.

(*c*) To consider in what respects the advantages derived by Corresponding Societies from their connection with the British Association may be increased or better understood, as, for example, in regard to (*a*) improved facilities for publication; (*b*) help in obtaining lecturers of recognised scientific standing.

This matter also was referred to the Corresponding Societies Committee.

(*d*) To consider the proposal made to the Council by the Corresponding Societies Committee : 'That all Corresponding Societies, while retaining the power to appoint any member of the British Association as their delegate, may instead (if they so choose) subscribe 1*l*. to the British Association and have the right to send two delegates during the year in which the subscription is received.'

It was explained that the Council had postponed consideration of this proposal until the opinion of the Conference of Delegates had been expressed.

Dr. F. A. Bather (John Evelyn Club) explained that the object of the proposal was to make it easier for the smaller societies to take part in the Conference.

Mr. W. M. Webb (Selborne Society) added that it might ensure the presence of delegates less likely to be distracted by other parts of the Association's programme than those who were already attending the meeting as ordinary members.

Other speakers deprecated the practice of representing two or more societies by the same delegate, and supported the proposal.

Professor H. H. Turner, F.R.S. (General Secretary), considered that the occasional presence of a second or a special delegate might be desirable, and that provision should be made for this.

On the other hand, Dr. C. Tierney (South-Eastern Union) thought it undesirable to provide for the payment of membership fees for delegates by the societies. Mr. Mark L. Sykes (Manchester Microscopical), Mr. Oke (Brighton), and others thought the proposal unnecessary.

The adoption of the proposal was formally moved by Mr. T. Sheppard (Yorkshire Naturalists' Union), and seconded by Sir Richard Gregory (President, Section L), but was negatived on a division (10 votes to 15).

(*c*) Arising out of the previous discussion, a motion was proposed by Professor J. L. Myres (General Secretary), seconded by Dr. E. H. Griffiths, F.R.S. (General Treasurer), and adopted as follows: 'To request the Corresponding Societies Committee to inquire as to the practice of the Association in regard to the distribution of the annual volume to Associated Societies, and to make recommendations.'

The Conference adjourned until Tuesday, September 12. At this first session 41 delegates were present, representing 49 societies.

The Conference adjourned until Tuesday . September 12. At this first
ber 12, at 2 p.m., Mr. W. Whitaker, F.R.S., in the chair; delegates present, 32, representing 45 societies.

Resolutions arising out of the business of the previous session were submitted by the Corresponding Societies Committee, and adopted as follows for transmission to the Committee of Recommendations:—

(*a*) To invite the scientific societies of Liverpool and District, on the occasion of the British Association's visit in 1923, to consider what further provision, if any, is desirable for co-operation between them in the advancement of science, as, for example, for scientific research, for the discussion of regional problems, and for the publication of results.

(*b*) To invite the delegates sent to the Conference by the Corresponding Societies to render any assistance in their power in making known, in their respective districts, the objects and methods of the British Association, and to communicate to the Secretary of the Association the names and addresses of scientific workers and others to whom the preliminary programme of the next meeting should be sent.

(*c*) To call the attention of the Council to the inadequacy, discontinuity, and occasional overlap of scientific bibliographies already issued; and to request the Council to consider what steps may be taken, by the Association itself or otherwise, to make more systematic provision for the bibliography of the departments of science represented in the sections of the Association.

(*d*) To request the Council to make known, in any way which may seem desirable, to the principal Government Departments the assistance which may be obtained by them through the local societies in scientific inquiries involving regional distributions.

(*e*) To call the attention of scientific societies to the necessity of retaining in all offprints from their publications the original numbering of the pages, and of providing full reference to the date, place, and title of the publication from which they are extracted.

(*f*) To call the attention of the Council to the value of the regional exhibit arranged for the Hull Meeting by the Yorkshire Naturalists' Union, and to suggest that it is desirable that such an exhibit should, if possible, be included regularly in the programme of the Annual Meeting.

(*g*) To inform the Conference of Delegates that the present practice of the Association is to present a copy of the Annual Report to each Society

sending a delegate to the Conference, recognising the practice by which one delegate sometimes represents more than one Society, and to recommend that in future no delegate be entitled to more than one copy, however many Societies he may represent; but that, if any Society desires a copy of the Report, it may be supplied at the reduced price of 10*s*.

(*h*) To apply to the Committee of Recommendations for a grant of 40*l*. for the preparation of the Report of the Conference and the annual Bibliography.

(*i*) To recommend that the Kent's Cavern Committee be not reappointed.

In regard to the last resolution, the President explained that there is at present no prospect of acquiring Kent's Cavern for the nation, and that certain subscriptions already offered to the Committee will be returned to the donors. Suggestions for more careful record of the results of any excavations which may be made by the present owner of Kent's Cavern were submitted by Mr. Mark L. Sykes (Manchester Microscopical), Mrs. Forbes Julian (Torquay), and Miss Nina Layard (East Anglia), and referred to the Corresponding Societies Committee.

Suggestions for co-ordinated work by the members of Corresponding Societies were received from the Committees of Sections of the British Association, as follows :—

Section C (Geology) recommends, as a subject for systematic research by the Corresponding Societies, the study of 'Peat Beds and Submerged Forests.'

Section H (Anthropology) commends to the Conference the proposal for an 'International Institute of Archæology in Rome,' in special regard to its scheme for a bibliography of archæological literature.

Section H (Anthropology) recommends that an urgent and useful work for local societies would be to assist the Folklore Society in collecting and organising the material still required to complete the information necessary for the contemplated new edition of Brand and Ellis' 'Vulgar Antiquities,' which has been long out of date.

Section K (Botany) calls the attention of the Conference to its Committee on a 'Primary Botanical Survey in Wales,' which desires local help in its researches.

A vote of thanks to the President of the Conference was moved by Dr. F. A. Bather, seconded by Dr. J. G. Garson, and carried unanimously. The Conference then adjourned.

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